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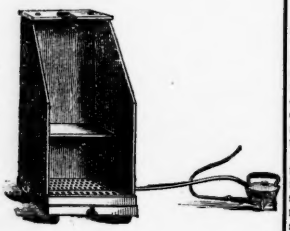

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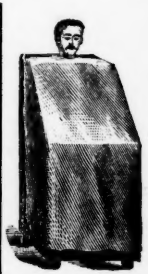

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THE

JOURNAL OF SCIENCE.

DECEMBER, 1884.

I. HYLO-IDEALISM: A DEFENCE.

(Communicated by ROBERT LEWINS, M.D.)

"Now call this One [essence] what you will, the result is the same, speculatively or practically. There may be certain degrading associations attached to the idea of substance, and certain exalted associations attached to that of spirit. But what difference can our associations make with respect to the real nature of things?"—LEWES'S *History of Philosophy*.

THE ingenious writer who has lately impeached Hylo-Idealism in the pages of this "Journal" places me in one respect at a disadvantage. He uses weapons which are not in my armoury, and which I have never been trained to wield. Freely confessing my inability to answer Mr. Billing in his own style, I shall proceed on the assumption that he is the most courteous and chivalrous of antagonists.

And, first, I must express the pleasure with which I find that, spite of all superficial differences, Mr. Billing and myself are really in perfect accord. The sentence which heads this article, and which he quotes from Lewes with apparent approval, is a complete answer to all insinuations respecting the "degrading" nature of Materialistic Monism. Elsewhere my seeming opponent virtually avows himself a Hylo-Idealist, remarking, with admirable clearness and force, that "the external world is as much a part of man as his own egoism." If the creed which I advance, "the fundamental principle of which is the Self," be really "subversive of all morals and order," and likely to make "the most disgusting license the rule and practice," surely Mr. Billing must consent to share the blame.

After this I need not seriously controvert any of the arguments brought forward, more especially as their premises are chiefly of the "intuitive" order, and as their logical sequence and the language in which they are couched are sometimes not very comprehensible to an ordinary intellect. Mr. Billing, however, represents some of them as "the commonest of surface reasoning," and I will not cavil at this description, which, perhaps, is not so completely irreconcilable with my own impression as may at first sight appear.

Having thus happily agreed with my adversary, I proceed to set forth in some detail a side of the Hylo-Idealist position which has scarcely received the attention it deserves. Such misconceptions as that of F. P. L., in your July number, are very natural, occur frequently, and therefore deserve to be carefully noticed and corrected. Mr. Billing falls into a similar mistake in his remarks on the oft-quoted saying that "Man is the measure of all things"; also when he alleges "analytical dissection, observation, and experiment," which "render Science possible"; also the facts of "geology and astronomy" as disproving Hylo-Idealism.

The difficulty is briefly as follows:—If "things" are "thinks," how can they be realities as well? If matter is ideal, how can ideas be the products of a material brain? I hope in the course of this article to dispel the perplexity, which really arises from an ambiguity in the use of the words "matter" and "things." I must begin by very briefly indicating the position and basis of Universal Scepticism.

Berkeley (by no means a Universal Sceptic!) argued that we have no right to attribute to the objects about us any existence independent of the mind. We ought not to assume that they are anything more than groups of our own sensations. Matter is a mere fiction. Byron writes in "Don Juan":—

> "When Bishop Berkeley said there was no matter,
> And proved it, 't was no matter what he said."

But we cannot treat the subject quite so lightly. Of course Berkeley *proved* no such thing; he proved only the insufficiency of the ordinary grounds for asserting that there is a reality underlying material phenomena.

Hume went a step farther. He showed that if we have no reason for believing in the existence of matter as distinct from material phenomena, neither have we any reason for believing in the existence of mind (or "spirit") as distinct from mental phenomena. He saw in the mind simply

a chain of feelings, or, as he calls them, of impressions and ideas, the latter being "copies" of the former. We shall see later that this word "copies" contains in itself the refutation of Universal Scepticism. But the Ego, considered as an Entity which manifests itself in "impressions and ideas," and links them into an organic whole, was to Hume as pure a fiction as Matter was to Berkeley.

It is not necessary that we should proceed beyond Hume. None of the great German philosophers has really turned his position, and if we desire to ground ourselves thoroughly we must return to Hume, and face his problem.

First, it must be admitted that Hume is quite right in considering the existence of the Ego as a pure assumption; but it can be shown that philosophy without assumptions is unthinkable. I do not, however, invite my readers to assume anything so debateable as the Ego. I ask them to assume only what the sceptic himself tacitly assumes when he begins to reason, and that is the *possibility of reasoning*. As I have elsewhere remarked, to reason in defence of reason is to beg the question,—to reason against reason is to commit suicide.*

Let us see what our assumption involves. It involves the belief that our ideas have some relation to objects, and our concepts to ideas;† the belief, that is, that it is possible to *remember*, and to *think*.

But *what* is the relationship which we have thus asserted? When we call to mind the concept, say, of a pin, we know that this concept is derived from various mental images of pins which we have formed from time to time. We know that these mental images were actual re-presentations of certain objects or grouping sensations, including hardness, brightness, slenderness, sharpness. Conversely, facts about pins not yet seen may be deduced from the concept,—as that they will prick, and will bend if subjected

* St. Augustine says "Everyone who knows that he doubts, knows something that is true, and is certain of what he knows. . . . . Therefore no one who can doubt at all ought to doubt that there is such a thing as Truth." Again, he says, "Men have doubted whether the vital force is of air, or of fire; but who doubts that he himself lives and remembers, and understands and wills, and thinks and knows and judges? Seeing that even if he doubts, he lives; if he doubts, he remembers why he doubts; if he doubts, he understands that he doubts; if he doubts, he wishes to be certain; if he doubts, he thinks; if he doubts, he knows that he is ignorant; if he doubts, he judges that he should not venture to assert."

† I use the term "idea" in the sense of a mental image of a sensation, an object, or a relationship; the term "concept" in the sense of a generalised idea, formed by the juxtaposition of two or more mental images, the rejection of the points in which they differ, and the retention of those which they have in common.

to sufficient pressure. Here we have a simple case of inductive and deductive reasoning.

We are also sure that the concept could not precede the idea, nor the idea the object. If it could be so, we should be able to remember things before they happened, and to reason about them before we remembered them. The future would be chronicled like the past.

The object, then, is not an accidental, but a *necessary*, antecedent of the idea and the concept. And this is equivalent to the assertion that the successive members of a chain of reasoning do not merely *succeed*, but also *influence* each other. They are not a mere series of dissolving views, which can be arranged in any order. There is an actual *genetic connexion** between them. But in this case they must be something more than passive pictures; for genesis implies action.

What, then, are we to say about feelings, states of consciousness, or (to employ a useful term) psychoses, since they are not to be spoken of as "passive pictures"? Evidently we must say that there is more in them than the mere appearance; because an appearance, *quâ* appearance, can only be passive: therefore we are forced to the conclusion that they are manifestations of some active Entity, some "*Ding-an-sich*," which shows itself in each member, and unites the whole together in a definite manner. I say Entity, rather than entities, because the *plexus* of psychoses called the mind forms in a very intelligible sense a unity, though the conception of unity is of course always relative, and there is no possible unity which may not, under some light, be considered as a multiplicity. The sentient and thinking "Thing-in-itself" (*noumenon*), at which we have at last arrived, may be termed the Ego.

Leaving this part of our subject for the present, let us turn to consider the relation between the Ego and its environment,—if, indeed, it *have* any environment. Having faced Hume's problem, let us next face Berkeley's.

We find that the sphere of consciousness may be roughly divided into two parts, which may provisionally (though inaccurately) be called the *inner* and the *outer*.

The inner includes mental and physical pains and pleasures, desires, volitions, ideas, and concepts. All these I shall group under the name of *feelings*. The outer is composed of objects and their perceived relationship. This outer region I may call the realm of phenomena.

* I use the term "genetic" in preference to the term "causal," as less suggestive of obsolete ideas.

Having established the existence of the Ego, we now proceed to ask whether there is anything besides the Ego? Is the world simply the product of the individual mind? Are objects mere mental phantasms, or are they manifestations of active entities which co-operate with the Ego? To answer this question we must separate them into three classes.

The majority of the objects which we perceive have no direct and constant relation to our mental and physical pains and pleasures, and do not obey our volitions. Neither do they seem to have any feelings of their own. Such are stones, gases, plants. These appear to act and react upon each other: I say *appear*, because I do not wish to assume at present that there is any reality corresponding to this appearance, or that a rock or a tree is anything more than a group of phenomena. If we investigate more closely, we notice that every action is followed by a reaction, and that the reaction always bears definite quantitative relation to the action. We noticed nothing of the kind when dealing with ideas and concepts, because neither an idea nor a concept admits of quantitative measurement.

But these apparent actions and reactions among objects are not necessarily accompanied by any feeling in the percipient, beyond the visual and mental one of perceiving them; that is, when you see one stone strike against another you do not feel hurt, unless your finger or some other part of your body be in the way.

But this brings us to the second class of objects, which consists, for each individual, of one member, or rather of a complexus of members. When any part of this complexus is touched, he feels; when one part of it touches another, he can distinguish two sensations. When it is injured, he is hurt; and its movements correspond with his volitions. This he names his *body*.

Let us enquire what he notes with regard to it. This plexus of *phenomena*, called body, is closely connected with a plexus of *feelings*, called mind. Take a very simple example showing this connexion—the prick of a pin. A bright, sharp, smooth, hard object, of a certain size and shape, is perceived by sight and feeling in a definite position with regard to that group of phenomena known as the finger. The skin is punctured; blood flows. A sharp pain is felt, and the hand is snatched away. But this is not the end of the matter. Several ideas have been generated,—the mental image of the pin, of the prick, of the blood, of the pain, and of the consequent movement; and these are henceforth

inseparably associated. When we have been pricked more than once they are generalised into concepts, and the sight or touch of a pin henceforth arouses a volition prompting us to avoid treading on it or sticking it into our finger. Here, according to previous admission, the idea of the pin is derived from the phenomenal object, and the concept from the union of two or more ideas. And if we shut our eyes no pin will be seen, and no visual mental image formed, so that the intervention of the eye is a necessary link in the chain. In like manner the pain of the prick is only an intensification of the sense of contact; and if the finger were not presented there would be no prick, and no pain, and therefore no subsequent idea of the pain. So that we have the two chains, of feelings and of phenomena, apparently interfering with each other; we have members of the one set standing in genetic relationship to members of the other. Indeed, as we have seen, this happens whenever an object is succeeded by its correspondent idea.

Two questions here suggest themselves. One is, the old and still unanswered query—Are the pin and the finger too mere mental phantasms, or have they any other kind of existence? The other is—Have we any reason to believe that either chain is broken; that, for instance, a phenomenon may be succeeded by a feeling, coming instead of another phenomenon? Or are the two chains collateral, the appearance of a link in the one being in some way essential to the appearance of a link in the other, yet being always followed by a link of its own kind? The only modifications that the individual can *see* in his body are the puncture and the flowing blood. But does the modification end here? Are there no other physical changes which might be rendered apparent to the senses, if there were anyone to trace them? These questions we can answer only by proceeding to the *third* class of objects.

The Ego is not alone with the body, and with the world of non-sentient objects. Other objects are perceived, resembling in all important particulars the percipient's own body, and appearing to act in a similar manner. When these act on one another *he* does not feel anything, but he cannot help inferring that *they* do; or rather, that feelings resembling his own are connected with these bodies which resemble his own. "How this inference is justified," says Prof. Clifford, "I do not pretend to say." * It cannot be justified by reason, but it is a procedure such as forms the very *basis* of

* In his Essay "On the Nature of Things in Themselves."

reason. We have seen that the formation of concepts is a necessary mental process, and no concept can be formed save by a perception of the analogy between two or more ideas. Concepts, of course, may be true or false, as the analogy is superficial or profound; in this case the more closely the analogy, or rather homology, is investigated, the more profound it appears.

At last, then, we have arrived at something which we can regard as an actual and active existence, independent of the individual Ego, but like it; in fact it is *another* individual Ego. And this inferred existence Prof. Clifford proposes to call the "eject," because, as he says, it is "thrown out" of consciousness.* This seems not a very happy term; however, I shall use it for the present.

All that I know of the "eject" I infer from my observation of that complexus of phenomena which is like my body, and which I call *his* or its body. Now, when I modify that complexus in any way—say by a pin-prick—I find (from signs which I learn to interpret analogically) that I have aroused a train of feelings, resembling those which are aroused when I prick my own finger. These feelings, like my own, are manifestations of some active Entity, and this Entity is not dependent on my mind for existence. The pin-prick, or, as I may call it, the *stimulus*, is genetically connected with this train of feelings, just as the feelings are connected with each other,—sensation with idea, idea with concept,—consequently it must be linked with them by some Entity, some "Ding-an sich," upon which I have been working. And evidently, when I modified the group of phenomena called the finger, I acted on a part of that Entity. That is, the human body is the manifestation of a "Thing-in-itself." Let us now get rid of the word "eject," and call the body and mind of my fellow-creature, taken together, the *project*.

By further investigation I find (in a way which I need not detail to readers of the "Journal of Science") that the phenomenal or physical chain is unbroken; that the stimulus is followed by the transmission along a nerve of a wave of disturbance; that this passes to the brain, occasioning there new and complex molecular motions, resulting in the sending outward of another nerve-wave, and finally in the contraction of a muscle. I also learn that every psychosis is accompanied by brain-waste; that if the brain be injured, or insufficiently fed, no psychoses are produced; and that if it

* Dr. Lewins has been in the habit of terming it "project," as a substitute for the Kantian "object."

be excited by stimulants the emotions are produced in excess, and the thinking powers, while at first heightened, are afterwards diminished. I find (in the words of Professor Huxley) that "the brain is the organ of sensation, thought, and emotion—*i.e.*, some change in the condition of the matter of this organ is the invariable antecedent of the state of consciousness to which each of these terms is applied." This statement is complete, with Prof. Clifford's important supplement that "some other change is the invariable *concomitant* of sensation, thought, and emotion. The two series—of psychoses and neuroses—are parallel so far as they run together, but they are not co-extensive. There is no psychosis without a corresponding neurosis, but there are neuroses without corresponding psychoses. The psychoses are concomitant with those neuroses only which take place in the brain, though genetically connected with others taking place in remote parts of the body. And neither chain is ever opened to admit the intercalation of a member of the other series.

When we find this mutual influence and parallelism between the two series, and reflect that, while each consists of manifestations of an active Entity, there is yet not a particle of evidence for imagining that there are *two* of these entities running parallel and interacting: when we further consider that this clumsy dualistic supposition can be traced to the fancies and dreams of savages, we shall soon be landed, willingly or unwillingly, in philosophic Monism. We shall be led to the conclusion that there is *one* "Ding-an-sich," of which both mind and body are phenomena; and that an act of this Entity which from one side appears as a change in the condition of the cerebral cortex, from the other side appears as a thought or emotion. *Why* there should be the two sides no one can say; but neither can anyone say why there should be the one side.

Dualism involves not merely the initial assumption of *two* Entities, but a long chain of assumptions, increasing as it proceeds in complexity, difficulty, and inconsistency. For instance, we have to imagine either that Spirit can act and be acted upon by pulls and pushes, or that Matter can act and be acted upon *otherwise* than by pulls and pushes, which, in ordinary experience, constitute the only mode in which it can make or receive impressions. On the former theory Spirit must have material attributes; on the latter Matter must have spiritual attributes. But Spirit with material attributes is conscious Matter; while Matter with spiritual attributes is corporeal Spirit. On either supposition one of the two Entities is superfluous.

The name of the Entity at which we have at last arrived is of very little importance, so long as we clearly recognise the facts. Dr. Lewins, the founder of Hylo-Idealism, terms it matter, and I adopt his nomenclature as the simplest and least misleading. To speak of "Spirit" with Hegel, or "Will" with Schopenhauer, seems to imply the necessary possession of mind or volition, and Herbert Spencer's "Unknowable" has more than a tinge of mysticism.

Not only may an animal exist in an unconscious state, but we are entitled to decide from analogy that a similar independent existence may be ascribed to objects which, so far as we know, are always unconscious, as plants and stones. The so-called "external" world is restored to reality, and, in spite of Mr. Billing's fears, geology and astronomy are quite at liberty to enter the service of Hylo-Idealism.

Every man's world is produced by the interaction of his nervous system with other matter; but it is the activity of the brain that brings the world into consciousness, and prescribes the forms of space, time, and causality under which it shall appear. It is the material brain that gives light and shade, form and colour, extension and solidity to the Cosmos; it is the material brain that procreates the beauty which we love and the sublimity which we worship. Change the human organism, and you have changed the visible and tangible Universe.

Schopenhauer expresses this with great clearness. "Suns and planets," he says, "without an eye that sees them, or an understanding that knows them, may indeed be spoken of in words; but for the idea, the words are absolutely meaningless. . . . . The existence of this whole [phenomenal] world remains ever dependent upon the first eye that opened, even if it were that of an insect. For such an eye is a necessary condition of the possibility of knowledge, and the whole world exists only in and for knowledge, and without it is not even thinkable."

C. N.

(CONSTANCE ARDEN.)

II. WÖHLER AND LIEBIG.

By Miss KATE WINDSCHEID.

THE names Wöhler and Liebig are so inseparably united that their friendship, which has found its fullest expression in their letters to each other, claims our sympathy to a high degree. The correspondence has been for the greater part preserved, and will probably be published ere long. Prof. Hofmann has made known a considerable number of the letters in his essay "In Memoriam of Friedrich Wöhler." By his kind permission I am enabled to make the English public acquainted with such parts of this in every respect highly interesting correspondence as refer to the deep-rooted love and respect which united the two friends, excluding all scientific topics.

At the time when the two men became acquainted Wöhler was Professor at the School of Industry at Berlin, and Liebig Professor at Giessen. They first met at Frankfort in the house of a common friend, and, though their intercourse was but short, they parted firmly convinced of having found a friend for life.

Liebig to Wöhler.

(Answer to a letter not preserved.)

"Giessen, February 11, 1829.

"Dear Doctor,—Your kind letter of January 20th, which I have duly received, has given me the greatest pleasure, as a proof of your lasting friendship. Be assured that I fully respond to your feelings, and that the few hours we spent together at Frankfort will ever be a most agreeable reminiscence to me. I am fully convinced that our friendship will never be subject to an interruption by the skirmishes we have had or may have again. . . . .

"I am sorry to hear that you are unwell. I have suffered for years from the same complaint, which forced me to put a stop to all work in my laboratory. You will learn to take care of yourself only when you shall have a family of your own, and I wish with all my heart that this may soon be the case. You will then attach more importance to your health, which must be precious to everybody that has a heart for Science. I look forward with joy to a line from you.

"Believe me unchangeably yours,

"LIEBIG."

Wöhler to Liebig.

"June 8, 1829.

"Dear Professor,—Mr. Poggendorff has communicated to me the contents of your last letter, and I am glad thus to find an opportunity of continuing the correspondence that was begun last winter. Really there must be a demon in the case, who takes a certain pleasure in bringing us into collision by our work, and in making the public believe that we are adversaries seeking for apples of discord. He shall not succeed, however. If you like we might begin jointly some chemical work, and publish the results under both our names. The choice of the theme I leave entirely to you. At the end of July I hope to go to Frankfort, and to stop at Giessen, in order to have the pleasure of being once more in your company."

Liebig to Wöhler.

"Giessen, June 12, 1829.

"Your letter of the 8th of June has given me great pleasure. I joyfully accept your proposal of treating in communion the same subject, and shall be most happy to see you here in July, when we can talk over the ways and means more closely."

Only a few months later the friends are enabled to publish the results of their researches. There appeared in 1830 the first work that bears at its head the joint names of Wöhler and Liebig ("On the Composition of Mellitic Acid").

In 1830 the Swedish *savant* Berzelius visited Germany, and Wöhler was most happy to bring his friend into connexion with his former teacher, to whom he was bound by the strongest ties of gratitude.

Wöhler to Liebig.

"Berlin, August 25, 1830.

"Only a few lines in haste, dear friend, to inform you that Berzelius has arrived here yesterday. We all wish that you might go by Berlin on your way to Hamburg. It would give great pleasure to Berzelius, who has charged me to beg you in his name to comply with our request."

Liebig to Wöhler.

"Giessen, August 28, 1830.

"Your idea that I should go by Berlin to Hamburg is very tempting, but impracticable. A Professor at Berlin can afford many things from which a Professor at Giessen must desist. Even my journey to Hamburg is a sacrifice in pecuniary respect. My heart is heavy at the thought that I shall not be allowed to pass some days with Berzelius, you, and our other friends, but I comfort myself with the prospect of seeing the former at Hamburg."

Liebig to Wöhler.

"Giessen, October 12, 1830.

"I am back again at Giessen ready for any work, and longing for news from you. My journey to Hamburg was successful in every respect. Berzelius has received me in the kindest manner, and has authorised me to communicate with him by letter. I only regret my opportunities for confidential conversation with him having been so few. He has quite won my heart by his amiable and unpretending ways, and I perfectly understand your attachment to him. Everything would have been perfect if you had been there."

Wöhler to Liebig.

"Berlin, October 13, 1830.

"Let me (first) thank you sincerely for the kind lines you sent me from Hamburg, which gave me all the more pleasure for their being written on the same sheet with those from a friend, who has long wished to make the acquaintance of a man for whom he entertains the highest consideration."

The following letter must be the answer to one written by Wöhler, which, however, is not to be found in the papers in hand. We can guess, though, pretty well at the nature of the "proposal" which Wöhler has made to Liebig, for in all the following letters (according to the German custom) the brotherly "thou" has taken the place of the conventional "you."

Liebig to Wöhler.

"Giessen, October 19, 1830.

"I wish I could sufficiently express the pleasure which your last letter has given me. I need not add that I most sincerely accept your proposal. I love you as if the ties which bind us had united us from our very youth, and I always did find it difficult not to speak in my letters to you the language of intimacy. You may be convinced that I am yours with all my heart, and that our friendship greatly contributes to my happiness. All I fear is that you will by-and-bye think less highly of me when you will have become aware how very limited is the knowledge I have acquired. You complain of the translation of Berzelius's writings, which, as you say, takes up all your time, and makes work of your own almost an impossibility. Dearest friend, I have long grieved to see that you waste your time on things which are unworthy of yourself, and I regret it all the more as I foresee that they will soon deprive me of your co-operation in our common work. To the deuce with your scribbling; and to the laboratory, where your place is, with yourself."

Wöhler to Liebig.

"Berlin, November 17, 1830.

"You scold me for over-burdening myself, dear friend, without considering that life is much more expensive here than at Giessen. Believe me, that if this were not the case translations and lectures might go to the devil for all I care. I undertook the task, however, at a period when I had much more time to spare and a very small income, and now I am in honour and out of gratitude for Berzelius bound to finish it."

The letter from Liebig to which Wöhler refers in the following lines is not to be found in their correspondence. Undoubtedly Liebig has again reproached his friend with working too much.

Wöhler to Liebig.

"Berlin, December 18, 1830.

"Was there ever anything more funny than the terms on which we stand? We are intimate friends, we work together, we behave rudely or amiably as the occasion offers,

and yet neither has a very clear idea what the other is like; though this is hardly astonishing, considering the lapse of time since we met. These thoughts occur to me while looking over your last letter, which contains reproaches that ought to provoke me if they were not wholly unmerited, and if your kind intention were not peeping out between the lines. All this, however, is necessary to keep up the humorous side of our friendship, and I am very glad that it should be so, and that Science should in some degree profit by it."

In 1831 Wöhler resigned, quite unexpectedly, his functions at Berlin. The reason for this sudden change may partly be found in disagreeable dissensions with his colleagues, and partly in his being overburdened with heterogeneous work, which hindered his indulging in scientific occupation. A school of industry being set up about this time at Cassel, he accepted the proposal of going there as a teacher, on condition that he should lecture on the same subject as at Berlin.

In December, 1831, he writes to Liebig:—"Everything is now most satisfactorily arranged here, and my position at the School of Industry will be the same as at Berlin. My heart ached when I was offered leave of absence for a whole year on condition that I should stay, but for honour's sake I could not act otherwise."

In 1832 Wöhler entered on his functions at Cassel; his innate love for experimental research had revived after a period of rest, and the letters of the two friends abound in new projects; they both know perfectly well that they are mutually dependent on one another.

In the midst of his scientific plans Wöhler is afflicted by a heavy stroke of fate. He loses his young wife after their having been united only for two years. In these hard times friendship is the goal which opens to the shipwrecked men. Liebig does not rest until he knows his almost despairing friend safe under his own hospitable roof; and now the two men work for the first time side by side, and the results of their efforts are deposed in the admirable work on Benzoic Acid.

Wöhler to Liebig.

"Cassel, August 30, 1832.

"I have now returned to my dreary solitude, and do not know how to thank you sufficiently for your love. I have

been supremely happy in working side by side with you, dear friend.

"I send you here enclosed the essay on Amygdalic Oil. The writing of it kept me longer than I thought it would. I beg you will read the whole with great attention, and especially to have an eye on the ciphers and formula. Please change without much ado what does not suit you. I often feel that something is not as it ought to be, but I cannot find the right expression for it."

In 1836 an important change took place in Wöhler's life. He was elected Professor of Chemistry at Göttingen, reaching thus the goal of his ambition—a Professorship at a German University. With renewed zeal Wöhler, seconded by Liebig, devoted himself now to scientific researches, and during the next twenty years a series of eminent discoveries delighted and astonished the world.

It is elevating, indeed, to behold the mutual endeavours of the two *savants* to investigate the realm of Nature. Their working in common only ceased when Liebig began to occupy himself with the application of chemistry to agriculture and physiology. He often regretted, in after times, having abandoned this sphere of action, and the sight of the brilliant discoveries which Wöhler was continually making awakened in his breast a feeling of regret. It is called forth by the consciousness of having exchanged a field of work where laurels bloom for one covered with thorns. His letters from the years 1857 to 1860, when he lived already as a Professor at Munich, fill us with deep sympathy.

Liebig to Wöhler.

"Munich, April 15, 1857.

"Your letters of the 5th and of the 15th sound in my ears like a fairy tale. My youth, years that are past, times that have died away, arise in me and remind me of the time when we worked side by side, joyfully and without envy. You have kept your mind unspotted, you are able to procure for yourself an enjoyment that is ever new, while I feel as though I were a deserter, a renegade who has forsaken his religion. I have left the highway of Science, and my endeavours to be of some use to physiology and agriculture are like rolling the stone of Sisyphus,—it always falls back on my head, and I sometimes despair of being able to make the ground solid."

And in November of the same year:—

"I admire you and your splendid works. How happy you are in your sphere! I am younger than you, and yet far duller. You are like that man in the Indian Fairy Tale out of whose mouth dropped branches of roses when he laughed, while I am condemned, together with the agriculturists, to pour water into the barrel of the Daneïdes. All I do is in vain; I am wasting my best forces, without being cheered by any success whatever."

A great many of the letters exchanged between Wöhler and Liebig refer to the travels they undertook at different periods. In 1843 Wöhler made his first journey to Italy. The enthusiasm which the southern world excited in him is reflected in a letter to Liebig, written on his return.

Wöhler to Liebig.

"Göttingen, October, 1843.

"I am safely back again at home, and do but regret that you have not been with me. I have derived both pleasure and instruction from this journey, the scenery as well as the people being entirely new to me. It is worth while making the voyage if it were only in order to enjoy that view of the blue Adriatic Sea from the heights of Optschina, and the drive down the mountain between olive and fig trees."

In 1846 and 1847 Wöhler went again to Italy, both times extending his journey as far as Naples. When he prepared to go for the second time to this beautiful country, he tried in vain to persuade Liebig to accompany him. "Your proposal to go to Naples," he writes in June, 1847, "is very tempting, but I am so inconceivably dull that I cannot make up my mind. I am tired of strange faces, and not inclined to worry myself by speaking French. After all, what good will it do to me to have looked into the crater of Mount Vesuvius?"

Wöhler to Liebig.

"Göttingen, July 22, 1847.

"It is actually a relief to me to find you too, for the time being, so very tired of chemistry. You cannot imagine how weary I am,—how sick of the whole concern. Some parts make me yawn when I think of them. What is the cause of this weariness? Have we, then, turned already so very old?

"At any rate it is high time for us to take a good airing, to bathe ourselves in the pure mountain air, and to keep away for some time from everything like chemistry. Your invitation to the Odenwald is very tempting, but Nature there is on too small a scale for me. If I am to recover I must have the sea and snow-covered mountains. Why won't you come to Naples with me? The beauty and splendour of that scenery are beyond all description, and to have stood on the brink of a volcano *is* worth something, for all you say."

In vain; Liebig is not to be persuaded, so Wöhler is forced to undertake the journey in company of another friend.

Wöhler to Liebig.

"Mentone, March 30, 1870.

"The heaven is grey to-day; the wind is in the east; from our windows we can hear the loud surging of the sea; swarms of sea-gulls seek for their prey on the dancing waves. In the end one gets tired of looking at all this, and is quite content to take up a book or to settle down to letter-writing. I am not allowed to go out to-day, having caught cold on a drive to Nice, on a day when the heat was very great. I won't tire you with a description of our journey, which was rather cold until we came to the sea, to Toulon, where we plunged all at once into an abundance of warmth and green leaves. There is hardly anything more beautiful than the railway road along the coast, with the view on the blue sea, with its bays and promontories. At Toulon we saw the first date trees, which stand out so clearly against the blue sky and the sea. Mentone is but a small town, built partly up the steep coast, and containing several good hotels, with beautiful gardens by the sea. In the early morning, when the sun has not yet quite risen and the air is clear, we can see quite distinctly from our windows the hilly coast of Corsica."

When Wöhler is by adverse circumstances denied the pleasure of travelling with Liebig, he flies to the Lake of Geneva, to his beloved Montreux. In letters to his friend he often speaks of that most beautiful spot "offering the view on the glorious blue lake and the mountains of Savoie."

In January, 1872, after having gazed from the window of his study, the garden covered with snow, and the leafless trees that look like broomsticks, he writes to Liebig:—

"It is quite warm already at Montreux, and the most glorious spring weather. I long for the blue lake and the beautiful mountains, and quite understand that Davy and Faraday should have loved this spot so well."

A slight indisposition which befel him in spring obliged Wöhler to put off the desired journey to the autumn of the same year. Liebig was prevented from accompanying him by the preparation for an academic speech.

Wöhler to Liebig.

"Vernex, September 18, 1872.

"It is four days since we have settled down here in one of the most comfortable boarding-houses, with a large garden close by the water. The mountains of Savoie are steeped in light, the lake is perfectly tranquil, and we enjoy with all our might this short space of untroubled existence. At noon, indeed, the heat is very great, and we are obliged to seek the shade of the big plane trees by the lake."

Liebig to Wöhler.

"Munich, September 22, 1872.

"Your letter, which pictures so vividly your residence at Vernex and Montreux, has given me great pleasure, as it clearly shows that the air, the lake, and the comfortable lodgings have strengthened your health. I am very sorry that we have not met this autumn. I had looked forward so much to your coming to see us on your way back."

Again and again the delight which Wöhler took in the contemplation of Nature is reflected in his letters to Liebig, breaking forth in exultation when he speaks of the South of Italy:—

"On the highest summit of the Blue Mountain stands the palace of Tiberius, in whose shade I ate splendid grapes and figs, while two brown-faced girls, the guides of our horses, danced the Tarantella to the sound of the tambourine. The view from there on the blue sea, on the gulfs of Naples and Salerno, on the promontory where Circe tried by her singing to seduce Ulysses, is something heavenly in its beauty,—words are not strong enough to describe this splendour."

Besides the repeated journeys to Italy, Wöhler visited England in 1835. Though the letter in which he describes his stay is not addressed to Liebig, but to Professor Dr. Hofmann, it may be yet of interest to an English public:—

"I shall always remember with pleasure the journey I made to England in 1835. We visited several technical establishments at Worcester, Birmingham, and Manchester, and travelled to Liverpool by railway, the only one that then existed in England, and which excited our wondering admiration. Faraday, who received us most amiably, and conducted us personally through several manufactories, had provided us with letters of recommendation. When we first visited him he took me for the son of the Wöhler he had heard of, because I was so thin as to look very young. In London we saw Prout, in Manchester the old Dalton."

Let us now dwell for a moment on those letters which speak of the mutual respect and love of the two *savants*. How sincere a friendship united them is proved by their readiness to give and to take advice. It is especially Liebig who consults his friend in every doubtful case. He publishes nothing without having first asked his opinion.

Liebig to Wöhler.

"Giessen, March 20, 1841.

"Your letter has been a great comfort to me. If you consider how great an influence you exercise on my ideas and on my work, how a mere point of interrogation from you sets me off on a train of thought, you can well imagine my joy on finding that your former experiences have led you to no conclusions that are contrary to those at which I have arrived. If your intelligence does not tell me that I am on the wrong track—and that was exactly what I wished to hear—I have courage enough to proceed.

"I have spent a whole day in developing to you my views on alimentation and respiration. You will see that they are contrary to everything which I have formerly admitted, but I am convinced of their correctness, and verily believe that they form the foundation of physiology and pathology."

How great a treasure Liebig possessed in the friendship of his companion is best shown by the remonstrances which Wöhler is never tired of making to his vehement friend. The following letter he addressed to him in the beginning of the quarrel between Liebig and Mitscherlich :—

"Cassel, March 3, 1834.

"I have just received a letter from Poggendorff, who urgently desires me to join him in begging you to leave your paper against Mitscherlich unfinished. Though he has communicated to me its contents only quite in general, I am horrified at the thought of the scandal you are about to begin. Granted that you are perfectly in the right, that scientifically as well as personally you have cause to complain, by doing this you stoop from the elevated position in which posterity will see you to a vulgar sphere, where the lustre of your merits is sullied. I hope you won't believe that I want to flatter you. Do not think that your attacks will harm Mitscherlich in the eyes of the world; his real merits, which even you must acknowledge, will always secure him an honourable place, and as in France, as well as in Germany, you have the repute of being quarrelsome, the unfavourable opinion will fall back on yourself.

"Transport yourself into the year 1900, and let us suppose that Mitscherlich has taken no notice of your attacks, and established his reputation still more firmly by successful discoveries. What would you think of yourself, what of him, in reading about the quarrels of the year 1834? What is the good of it? You will annoy Mitscherlich a little, amuse the public, embitter your life, and ruin your health. Believe me that people will only look upon these contests as the outbreaks of a paltry spirit. Good-bye, dear friend, and don't be angry at my sincerity."

Wöhler to Liebig.

"Göttingen, March 9, 1843.

"To contend with Marchand or with somebody else will do you no good whatever, and be of little use to Science. It only makes you angry, and hurts your liver.

"Think of the year 1934: what will it matter, then, whether we have lived in peace or in enmity? who will then care for your quarrels, for the sacrifice of your health and spirits, in the cause of Science? Nobody; but your ideas, the new facts which you have discovered,—that is what will rest in the memory of men, what will always be acknowledged."

A quality especially characteristic of Wöhler, and which rendered him especially dear to his friends, was his perfect modesty. With the utmost energy he refuses what he

believes to be unmerited distinction, and in measure as he keeps his own self in the background the merits of others are brought forward by him.

At the time when Liebig had published the Essays in which he defends his views on Bacon, Wöhler writes to him :—

"I have read them with great pleasure and with real admiration of your talents, which manifest themselves also in this line. It is not want of interest, but want of practice, which makes me slower than you in entering into such questions. The power of imagination is tolerably developed in me, but as to reasoning I am painfully slow. Nobody was less intended by nature for a critic. I do not possess the least mathematical (faculties) and philosophical faculties, though I flatter myself of having a tolerable mechanism for observing in my brain."

Gradually the day has begun to decline. "The shades of eve are falling fast." The future lies no longer like an unbounded plain before the eyes of the two friends, and in their letters to each other is expressed the foreboding of the coming night.

Liebig to Wöhler.

"Munich, January 2, 1869.

"I cannot begin the new year without expressing the wish that heaven may grant us the joy of spending the few years we have still to live in untroubled friendship. The increasing signs of old age warn us to put our houses into order. We are rolling on an inclined plane, and the end is not far off, but nothing will ever change our hearts."

A year later Wöhler writes to Liebig :—

"Göttingen, May 2, 1869.

"Life seems to me like a bad comedy; it bores me, and yet I do not like to leave the theatre, because I always hope that something better may come. 'Give me back my youth,' says the poet in Faust."

With the following words Liebig greeted his friend at the close of the year 1872, when the shadows of the coming separation were already gathering round them :—

"Münich, December 31, 1872.

"I cannot let the old year pass away without expressing the most heartfelt wishes for your welfare and that of your family. We shall not many times more be able to wish each other a happy new year; but even when we shall have died the ties which bind us will unite us, in the remembrance of men, as an example of two *savants* who struggled without envy in the same sphere, ever united by the closest friendship."

Wöhler to Liebig.

"Göttingen, October 30, 1872.

"Many thanks for your last letter, from which I learned that you are pretty well. It is a great bore that in old age one is obliged to attach so much importance to the state of one's health. Thirty, forty years ago nobody ever thought of asking the other whether he felt well. In youth we cannot imagine that life can ever come to an end, and it would be just the same in old age if the decrease of strength did not remind man of his approaching end."

On April 3rd, 1873, Wöhler received the last letter from his friend, whom he outlived for ten years:—

"I meant to have written to you yesterday, but I had a wakeful night, and spent the day on the sofa thinking of you. Is it possible, I wonder, to die in old age from want of sleep, without any acute illness? The lamp goes out gradually, when the body is no longer fortified by the vegetative life it leads during the night."

On the 18th of April the news of Liebig's death reached Wöhler at Hanau.

The correspondence between Wöhler and Liebig, of which fragmentary letters have been produced here, embraces a period of more than fifty years, and fills us with sincere admiration for them. Not a word in these hundreds of letters that they might wish not to have written; unselfish friendship, true love of mankind pervade these leaves; and, besides, they give us a clearly defined picture of the two friends so widely different in character, and yet so closely united.

Liebig a fiery, impetuous spirit, seizing with enthusiasm

every new idea, obstinately defending his conviction, yet grateful to whoever convinced him of his error.

Wöhler cool, deliberate, expressing his opinion, but after elaborate examination which seems to exclude the possibility of his being mistaken.

Liebig nervous, flying into a passion at a slight provocation, and consequently often engaged in violent controversy.

Wöhler passionless, imperturbable, a sworn enemy of quarrels and disputes.

The qualities, however, which they both possessed in an equally high degree were love of truth and loyalty. Is it then astonishing that between these two men, that were so entirely dissimilar, there should have blossomed up a friendship which both reckoned among the greatest blessings of their lives. Their mutual attachment, which was based on the highest personal respect, has become to all who have been granted the favour of knowing them more intimately a model for emulation.

III. ON THUNDERBOLTS.

By Colonel the Honourable ARTHUR PARNELL (late Royal Engineers.)

IN regard to the true meaning of the expression which forms the title of this paper a very erroneous idea seems to be prevalent, and especially among men possessed in other respects of scientific attainments. To illustrate this statement I would refer to the following instances. In "Symons's Meteorological Magazine" for August, 1880, the editor, Mr. G. J. Symons, F.R.S., then President of the Royal Meteorological Society, says:—"Thunderbolts do not exist. I have been trying to find one for a quarter of a century, and have always failed." At a meeting of the North of England Mining Engineers' Institute at Newcastle-on-Tyne, in December, 1883, at which I was present, Professor Lebour, of the Durham University College of Physical Science, informed the audience that a thunderbolt was a

piece of iron pyrites. At a meeting of the Royal Institute of British Architects held in January, 1884, Professor D. E. Hughes, F.R.S. (a member of the recent "Lightning Rod Conference"), in discussing a paper I had just read, said that he had long been trying to find a thunderbolt, and that he should much like to see one. And in an anonymous article "On Thunderbolts," in the "Cornhill Magazine" for November, 1884, a writer devotes several pages in attempting to demonstrate that thunderbolts have no existence.

Now, in all these cases the speakers or writers have taken for granted that a thunderbolt is the same as a *thunderstone*, *viz.*, a material projectile fabulously supposed in some of the darker ages to have been occasionally emitted by a lightning stroke. On what grounds such a confusion of terms should have been permitted to develope itself among men whose special business it is to keep the public rightly informed on such matters baffles my comprehension. For it is only necessary to turn to any standard English dictionary in order to obtain a correct knowledge of the real signification of the old English words *thunderbolt* and *thunderstone*.

Perhaps the readers of the "Journal of Science" will pardon me if I here adduce *verbatim* extracts from a few of these classical mines.

(1). JOHNSON'S DICTIONARY, Todd's Edition, 1827.

Thunderbolt. "Thunder and bolt as it signifies an arrow. Lightning. The arrows of Heaven."

Thunderstone. "A stone fabulously supposed to be emitted by thunder."

(2). WEBSTER'S DICTIONARY, 1841.

Thunderbolt. "A shaft of lightning; a brilliant stream of the electric fluid passing from one part of the heavens to another, and particularly from the clouds to the earth."

Thunderstone. A stone, otherwise called *brontia*.

(3). CRAIG'S DICTIONARY, 1848.

Thunderbolt. "A brilliant stream of the electric fluid, particularly if acting in a direction towards earth."

Thunderstone. "A stone fabulously supposed to be emitted by thunder; they are a crystallised iron pyrites of cylindrical form found in all chalk beds."

(4). CHAMBERS'S DICTIONARY, 1877.

Thunderbolt. "A shaft of lightning (particularly if

passing in a direction towards the earth); figuratively, something sudden and irresistible."

Thunderstone. "A variety of crystallized iron pyrites."

(5). WORCESTER'S DICTIONARY, 1881.

Thunderbolt. "A brilliant stream of lightning. A bolt of lightning."

Thunderstone. "A stone fabulously supposed to be emitted by thunder. Crystallized iron pyrites."

Probably these quotations will be considered sufficient to justify the charge I have brought forward, and to render it obvious that a thunderbolt is in reality merely a lightning stroke. I propose, however, to submit, in addition, some quotations from our two great wells of English undefiled, *i.e.*, The Bible and Shakespeare's Plays, with a view of showing that the lexicological interpretation which I have just enunciated is no mere pedantic fancy of the philologists who compiled these dictionaries. In the Scriptures the term is only once mentioned; but that once is cogently explicit. In Psalm lxxviii., 48, we read: "He gave up their cattle also to the hail, and their flocks to hot thunderbolts." That the translators intended to use this expression as synonymous with lightning strokes is clearly proved by the marginal correction which substitutes "lightnings" for "hot thunderbolts."

Shakespeare uses *thunderbolt* or *bolt* in the ten following instances.

Tempest, II. 2 [Caliban *loq.*]. "This is no fish, but an islander that hath lately suffered by a thunderbolt."

As You Like It, I. 2 [Celia *loq.*]. "If I had a thunderbolt in mine eye, I can tell who should down."

Henry IV. (Pt. I.) IV. 1 [Hotspur *loq.*]. "Come let me take my horse, who is to bear me, like a thunderbolt, against the bosom of the Prince of Wales."

Julius Cæsar, IV. 3. [Brutus *loq.*]. "Be ready, gods, with all your thunderbolts; dash him to pieces!"

King Lear, III. 2 [Lear *loq.*]. "You sulphurous and thought-executing fires, vaunt couriers to oak-cleaving thunderbolts, singe my white head!"

Antony and Cleopatra, II. 5 [Cleop. *loq.*]. "Some innocents 'scape not the thunderbolt."

Tempest, V. 46. [Prospero *loq.*]. "To the dread rattling thunder have I given fire, and rifted "Jove's stout oak with his own bolt."

Measure for Measure, II. 2 [Isabella *loq.*] "Merciful heaven! thou, rather, with thy sharp and sulphurous bolt, splitt'st the unwedgeable and gnarled oak than the soft myrtle."

Coriolanus, V. 3 [Volumnia *loq.*]. "To tear with thunder the wide cheeks o' the air, and yet to charge thy sulphur with a bolt that should but rive an oak."

Cymbeline, V. 4 [Jupiter *loq.*]. "How dare you, ghosts, accuse the thunderer, whose bolt you know, sky-planted, batters all rebelling coasts?"

I would suggest that in no one of these extracts does Shakespeare give the reader the idea of any other action than that of an ordinary lightning stroke, or of the irresistible force that (with the utmost physical accuracy) such a stroke may figuratively be supposed to typify. But to render it more certain that this master of the English tongue knew well the difference between such a stroke or bolt of force as I contend he has in the foregoing sentences been alluding to and the material bolt of which a thunderstone might convey the impression, I will now give two instances in which the poet actually mentions the latter term.

Cymbeline, IV. 2 [Guiderius *loq.*] "Fear no more the lightning flash." [Arviragus *loq.*]. "Nor the all-dreaded thunderstone."

Julius Cæsar, I. 3 [Cassius *loq.*]. "And thus unbraced, Casca, as you see, have bar'd my bosom to the thunderstone."

Moreover, if my researches be accurate, Shakespeare, throughout his Plays, only makes twelve allusions to strokes of lightning; in no less than ten of these he uses the term *thunderbolt* to set forth his meaning; and only in two of them does he employ the expression *lightning*. He mentions this last word altogether on fifteen occasions, of which eleven are in connection with the principal physical element of lightning, *viz.*, light; two are in respect of its heating qualities, and two (as aforesaid) to denote the stroke or explosion of which lightning is usually the accompaniment.

In regard to the use of the term *thunderbolt* to signify a lightning stroke, by writers on physical science, I am under the impression that during the last century, and long after a knowledge of electricity had spread over the world, the word was by no means infrequently availed of, but at present I can only lay my hands on two examples. The

first is a very important one, and consists of a *dictum* by the famous Dr. Joseph Priestley, F.R.S., in his "History of Electricity," 4th edition, London, 1775. Speaking of the analogy between a lightning flash and the electric spark, he observes: "It was Dr. Franklin who first proposed a method of verifying this hypothesis, entertaining the bold thought" . . . "that pointed iron rods fixed in the air, when the atmosphere was loaded with lightning, might draw from it *the matter of the thunderbolt*, and discharge it without noise or danger into the immense body of the earth. [Hist. p. 164]. It is clear that the "matter" here alluded to is the electricity causing the lightning stroke, or thunderbolt. The other case is the account of a lightning stroke at Holbeck, near Leeds, on September 1st, 1672, when a child (playing with others) was killed, his companions being badly burnt. The narrative is supplied to the "Philosophical Transactions of the Royal Society" (xxii., 578) by Mr. R. Thoresby, F.R.S., who quotes the parish register which records that the child was "slain by a *thunderbolt*." It is abundantly clear from the context that there was no stone or material bolt present, and that neither Mr. Thoresby nor the parish authorities had the faintest idea of any other agency than that of an ordinary lightning stroke.

But perhaps the most curious circumstance connected with the mistaking of thunderbolts for thunderstones is the complete manner in which the expressions employed by the French are ignored. It is hard to imagine that any man should allow himself to be accepted as an authority on the science of lightning strokes who had not studied French works on the subject, but it is certain that any such student could not but have noticed that the French almost invariably use the term *la foudre* to designate a *stroke* of lightning, whilst for lightning itself they employ a totally different word, *viz.*, *l'éclair*. They thus differ entirely from the slip-shod English practice of the present day of adopting the term *lightning* both for the stroke and also for its flash or luminous accompaniment. But what is the English translation of *la foudre*—the translation given by every French dictionary? Why, nothing else than *thunderbolt*. It would appear then that, whilst originally in both languages a scientific distinction was maintained between the stroke and its accompanying light, such distinction has in recent years been permitted in the less scientific country to drop, and that, on the strength of this negligence, English men of science have lapsed into a complete ignorance of the

existence in their language of a word corresponding to *la foudre* in the French tongue. To show again that the term *thunderbolt* is not a dictionary-maker's refinement for the translation of *la foudre*, I beg to call attention to the English version of Arago's "Meteorological Essays," compiled by the late Sir Edward Sabine, President of the Royal Society (London, 1855.) In this translation "la foudre" is repeatedly rendered by the word "thunderbolt." For instance, "Seneca defined *thunderbolts* as lightnings that reach the ground" [p. 24 abridged]. "Maffei, Chappe, and others, deem that" . . . "*thunderbolts* are almost always elaborated on the ground." [p. 101.] "*Thunderbolts* develope by their action, in the places where the explosion takes place, often smoke, and almost always a strong odour." [p. 62] "I admit, without reservation, the existence of ascending *thunderbolts*." [p. 176]. And, lest it should be conceived that the term *la foudre* was used by the French for thunderstones as well as for thunderbolts, I may here mention that their expression for thunderstone is *météorolithe*, a word which seems to give the so-called thunderstone what is probably its true scientific *status, viz.*, that of a meteorite or aerolite.

It may now, however, reasonably be asked whether, supposing the word "thunderbolt" to be the proper English term for a lightning stroke, it is even then an expedient one to restore to general use. In reply, I should be inclined to give my opinion in the negative. An erroneous meaning having once been attached to it by scientific authorities, there would probably be some difficulty in rehabilitating it with its correct signification, and I think that the expression *lightning stroke* would, for the present, and until our knowledge of terrestrial dynamics becomes enlarged, be more fitting for general adoption. But I would submit that no objection should ever be urged to the employment by educated men of the legitimate, old-fashioned term *thunderbolt*, should they desire to use it. And I would urge this the more particularly inasmuch as I think that the word represents with scientific accuracy the principal characteristics of all lightning strokes. These appear to me, so far as my researches have extended, to be first a clap of *thunder* of exceeding loudness, and secondly a figurative *bolt* or arrow of irresistible force piercing or rending the materials with which it comes in contact. There are on record several undeniable lightning strokes or thunderbolts in which no lightning has been seen; but I doubt if any strokes can be found in which no thunderclap was heard. And as regards

the second characteristic, it is certain that the records of mechanical force or violence on the part of strokes greatly outnumber those of heating action. Probably the term which eventually will be found to combine precision and simplicity in the highest degree will be *earth stroke;* for there is an overwhelming mass of evidence to show that lightning strokes spring from the earth, and that the direction of their force is invariably upwards. In fact, Nature in this matter amply testifies to the theory deduced from laboratory experiments by the great philosopher Symmer 125 years ago.

In conclusion, I would observe, in regard to the article in the "Cornhill Magazine" already alluded to, that it seems a little remarkable that any man of ordinary reasoning power should deliberately write a paper in a periodical of reputation purporting to prove that thunderstones had no existence. Who ever for a moment doubted the fact? He finishes his contribution with a notice of lightning rods, and eulogises what he considers to be their beneficent action. It is not easy to see what thunderstones have to do with these apparatus; but the writer, after a misleading account of their origin, winds up his dissertation on them with the words, "from the moment the thunderbolt was safely dead and buried." A more shallow scientific production than this paper it would be difficult to conceive, even among the contents of popular magazines usually devoted to the interests of fiction.

November 6, 1884.

IV. AN EXEGESIS OF DARWINISM.

By Oswald Dawson.

UPON the 24th of November last—that is, one week ago—the "Origin of Species" entered upon the second quarter of a century of published existence. I propose to celebrate the event by writing an exegesis; sentimental regard for this period of years, as for a silver wedding, being, however, perhaps less incentive to now

undertake the task than the fact that the colossal statue of Mr. Charles Darwin, by Mr. Boehm, which is to be erected in the British Museum (Natural History), is nearly finished. At such an hour a definition of Darwinism, sufficiently succinct for mnemonical purposes and amply substantiated by copious quotations, may be especially useful.

The word Darwinism has divers significations, and probably to many persons a somewhat indefinite one. It may invoke the image of "our cousin the gorilla" rubbing off its tail by friction, or elicit a taunt that we cannot specify the whence of the nebulæ. Some persons identify Darwinism with Natural Selection, others with Evolution, while Mr. Darwin's approved epitomiser warns us that "we must not confuse the Darwinian theory with Evolution;" for indeed we "cannot, strange as this may seem, call Charles Darwin an Evolutionist" when rightly employing that term. In a pamphlet on "England and Egypt" Mr. B. Fossett Lock, of the Positivist School, "analysed the Liberal party . . . into four groups, . . . Whig, Nonconformist, Darwinian, and Positivist." Long ago "the expression 'Darwinism' (as employed for example by the poet Coleridge when writing on Stillingfleet) was accepted in England nearly as the antithesis of sober biological investigation." Sir Wm. Thomson "out-Darwined Darwin" in suggesting the dispersal of living germs from another planet. Prof. Huxley doubts if he can ever have seen one of Dr. St. George Mivart's "absolute and pure Darwinians" alive. According to one critic Prof. Huxley "is much more Darwinian than Mr. Darwin himself;" according to another critic "Darwin himself is not a good Darwinian," inasmuch as "Darwinism is becoming Owenism." To these significations of Darwinism must be added the following definition:—

From one to at most ten (*vide* Section 1, *infra*) pre-Cambrian (v. 2, *infra*) presumably unicellular organisms (v. 3), created (v. 4) and vivified by the Creator's breath (v. 5), have arisen without any subsequent interference (v. 6), and we can hardly believe otherwise than without beneficent guidance (v. 7), the structures and the bodily and mental (v. 8) activities of all organisms, by the accumulation, mainly by Natural Selection (v. 9), of variations so minute as to be appreciable only to well-trained eyes (v. 10), occurring in correlation but not in co-ordination with related variations (v. 11) affecting single individuals (v. 12), inducing intermittent modification (v. 13), and being determined in their nature by an innate idiosyncratic plasticity (v. 14), the chief ultimate cause of which is probably the accumulating

action of changing conditions upon the parents, but more especially remoter ancestors, of the varying individual (v. 15).

1. "It is immaterial whether or not it be accepted" that "some one primordial form" or several forms commenced the career of life upon this earth.—("Origin of Species," * p. 425). Analogy, which "may be a deceitful guide" (p. 424), and Maupertuis's axiom of least action (p. 423, i., 13) favour the former view. Mr Darwin "believes that animals are descended from at most only four or five progenitors, and plants from an equal or lesser number" (p. 424), though "he does not wish to lay much stress upon the greater simplicity of the view of a few forms or of one form only having been originally created, instead of innumerable miraculous creations having been necessary at innumerable periods" (i., 12).

2. It was not that each geological epoch was honoured by the vivification of a prototype; the several prototypes were "some few beings which lived long before the first bed of the Cambrian system was deposited" (p. 428).

3. "A cabbage may have been the parent plant, a fish the parent animal—it may have been a whale," remarked the "Athenæum;" and I once read that in Church Congress a Fellow of the Royal Society had solemnly urged the claims of the elephant. And, indeed, what more natural supposition than that Mr. Darwin assumed a plurality of prototypes to account for the wider differences between the several sub-kingdoms? It appears, however, that they were unicellular organisms, for firstly, in the same paragraph in which he contends for the evolution of "each great kingdom, such as the Vertebrata, Articulata, &c.," on embryological (and other) grounds, he remarks that "all organisms start from a common origin; . . . the germinal vesicle is the same" (p. 425): secondly, enquiring how could differentiation succeed the dawn of life, he says, "Mr. Herbert Spencer would probably answer that as soon as simple unicellar organism came by growth and division to be compounded of several cells," &c. (p. 100); the implication being that Mr. Darwin himself starts with simple unicellar organism: thirdly, if the Foraminifera "could be proved to have come into existence during the Laurentian epoch" the circumstance would be fatal to "my view" (p. 308). Why then did Mr.

* Sixth Edition, with Additions and Corrections to 1872. The title will be omitted in subsequent references. Pages preceded by the numerals i. or ii. refer to the first or second volume of the "Variation of Animals and Plants under Domestication," Second Edition, revised, 1875.

Darwin offer us the alternative of a plurality? and further specify "only four or five progenitors" for animals? He writes:—"If at the first commencement of life many different forms were evolved, . . . we may conclude that only a very few have left modified descendants. For as I have recently remarked in regard to the members of each great kingdom, such as the Vertebrata, Articulata, &c., we have distinct evidence in their embryological, homologous, and rudimentary structures, that within each kingdom all the members are descended from a single progenitor (p. 425). Again, certainly we ought not "to believe that innumerable beings *within each great class** have been created with plain, but deceptive, marks of descent from a single parent" (p. 423). "But as the members of quite distinct classes have something in common in structure, and much in common in function, analogy would lead us *one step farther*, and to infer as probable that all living beings are descended from a single prototype" (i., 13); a comparatively precipitous "one step." In the first edition of the "Origin of Species" we were informed that "at the most remote geological period, the Earth may have been as well peopled with many species of many genera, families, orders, and classes, as at the present day" (p. 126; *cf.* 6 ed., p. 97).

4. "Mr. Darwin does not inform us whether he believes the Creator made the original progenitor of all living beings, and then breathed into it the breath of life, or whether it was produced spontaneously without life, and then life was breathed into it. If the former, then we have the Creator making, breathing into, and dropping into the water, the lonely protozoic Adam, that is to be 'the father of all living,' a microscopic gelatinous globule, the single tenant of a boundless ocean." (See, however, p. 422, and ed. i., 466. *Cf.* ed. 6, 410, and i., 12, which seem to imply literal creation.

5. See last sentence of "Origin of Species," which is not only "so distinctly anthropomorphic," but somewhat *theological*. Mr. Charles C. Cattell, secretary of the Darwin Institute, Birmingham, shall speak on this matter. "The early works of Darwin contain a phrase, 'Life breathed by a Creator into a few forms,' and Moses says of Adam, 'God breathed into his nostrils the breath of life.' On this slender foundation is built the immense superstructure that Darwin and Moses are in agreement."

The previous collation of passages concerning the nature

* The italics are mine throughout unless otherwise specified.

of the prototypes should prepare us to encounter ambiguity concerning their origin; and indeed one passage, if literally construed, upsets the special vivification notion. Mr. Darwin thought that "it accords better with what we know of the laws *impressed on matter* by the Creator, that the *production* and extinction *of the past* and present inhabitants of the world should have been due to secondary causes" (p. 428). He tells us that "Science *as yet* throws no light on the far higher *problem* of the essence or origin of life" (p. 421). "How a nerve comes to be sensitive to light hardly concerns us more than how life itself originated" (p. 144). The patent ambiguity, transparent scepticism, and naked indifference of these passages would not gratify anyone who had performed the breathing feat in question.

Assuming that the courageous Mr. Henry G. Atkinson is mistaken in believing that Mr. Darwin's invocation of a Creator to start life upon earth was a mere subterfuge, let us enquire whether or no that vivification endowed the prototypes with anything more than life—and perhaps prestige; was it the cause of the "innate variations" (p. 115), "innate wide flexibility of constitution" (p. 114), and "innate tendency to new variations"? This question will be answered in section 15. Meanwhile it will suffice to remark that Mr. Henry G. Atkinson is not the only traducer of Mr. Darwin respecting this vivification matter. From a diametrically opposite quarter, namely from a current "Dublin Reviewer," we have a questioning of Mr. Darwin's sincerity; while, to turn to avowed applauders, in the second "Agnostic Annual," just issued, the Secretary of the Darwin Institute, Birmingham, remarks that "The *early* works of Darwin contain a phrase, "Life breathed by the Creator into a few forms; . . . Those who quote Darwin as giving a supernatural personal origin to life should be reminded that in his 'Descent' he describes the enquiry as to 'how life itself first originated' as 'hopeless,' and says that the solution lies in the 'distant future,' if it is ever to be 'solved by man.'" The president of that institute classifies "the late Charles Darwin as one "of the *genus Humbug*," of a variety whose characteristics include "entire unbelief in the supernatural *at heart*." The editor of the "Annual" affirms that "the origin of life is untraceable, and absolutely inconceivable."

6. "As all the forms of life are the lineal descendants of those which lived long before the Cambrian epoch, we may feel certain that the ordinary succession by generation has never once been broken" (p. 428).

7. "However much we may wish it, we can hardly follow Professor Asa Gray in his belief 'that variation has been led along certain beneficial lines,' like a stream 'along definite and useful lines of irrigation.' . . . On the other hand an omnipotent and omniscient Creator ordains everything and foresees everything" (ii., 428). "The understanding revolts at such a conclusion" as that the origin of species is "the result of blind chance," "whether or not we are able to believe that every slight variation" was foreordained. ("Descent of Man," p. 613.) That an omniscient Creator foresees everything is a truism; we may also allow that an omnipotent Creator ordains everything. A third attribute—all-wise, and a fourth—all-beneficent, are, however, essential to a God proper. Neglect to exercise power solves the problem as easily as Mr. John Stuart Mill's tentative "hypothesis that the Creator is a being of limited power," displaying "benevolent design limited by obstacles," as he himself worded it in a recently-published letter to Mr. R. Pharazyn. It behoves me to marshal the evidences of Mr. Darwin's theism, and his indictment of the wisdom and beneficence of God.

Three quotations face the title-page of the "Origin of Species." The first is from a "Bridgewater Treatise"; the second from the "Analogy of Revealed Religion"; the third is a recommendation to obtain proficience in science and divinity. If Mr. Darwin entertained the views to which he gave such conspicuity, he thought that divine power is exerted in the establishment of general laws; that an intelligent agent effects natural results continually or at stated times; that a man cannot be too well studied in the literal book of God's word. Professor Asa Gray inferred that "the most candid of men" adopted the Whewell and Butler quotations as "postulate mottoes," and the Rev. Joseph Cook regards the concord of "the foremost naturalist of our times and the greatest modern Christian apologist" as "a fact in which much solace for timid Christians, and much taming anodyne for audacious small philosophers, lie capsulate." "This renowned passage has become in a new degree famous by being adopted through numberless editions as the postulate motto on the title-page of Darwin's 'Origin of Species.' It stands there as a headlight." This is quite natural inference if we believe the motto was adopted by one "whose greatest praise is that he was, before and above all things, *an honest man* (the italics are not mine, but a *Westminster Reviewer's*); by one "possessed of a certain intense and almost passionate honesty by which all his

thoughts were irradiated as by a central fire." A similar inference, however, is unwarrantable respecting the Bacon quotation. When we take the recommendation to obtain proficience in "the book of God's word . . . divinity" in conjunction with the implication that the impression that the doctrines of the "Origin of Species" would be subversive of revealed religion would be transient, and that this transiency was a matter for satisfaction (pp. 421, 422), we can comprehend why the *Quarterly Reviewer* should affirm that "Mr. Darwin writes as a Christian, and we doubt not that he is one."

In the study of his home at Down during the afternoon of Wednesday, the 28th day of September, 281 years after Bruno was burned, Charles Darwin, in the presence of Dr. Ludwig Büchner, Dr. Edward Aveling, and Dr. Francis Darwin, declared that he had given up Christianity when forty years of age. Visitors to Westminster Abbey may ascertain that Mr. Darwin's fortieth birthday anniversary was a decade anterior to 1859. The Bacon and Leibnitz quotations being, therefore, calculated to mislead; we may not, I think, consider that Mr. Darwin "postulated" theism on the strength of the Whewell and Butler quotations. Was he, then, Theist by induction? In answering this question in the negative I may refer to his repeated comments on the supplanting of indigenes by intruders, though the former "are commonly looked at as specially created and adapted for their own country" (p. 89); to his quotation of "the remarkable words of Helmholtz, whose judgment no one will dispute," concerning the imperfection of the human eye; to his exclamation, "can we consider the sting of the bee as perfect?" "can we admire the production, for [a] single purpose, of thousands of drones, which are utterly useless to the community for any other purpose?" or the elaboration of dense clouds of pollen by our fir trees, so that a few granules might be wafted on to the ovules (pp. 163, 164); to his remark that "To grant to species the special power of producing hybrids, and then to stop their further propagation by different degrees of sterility, not strictly related to the facility of the first union between the parents, *seems a strange arrangement*" (p. 245); to similar remarks as to prepotence in reciprocal crosses (p. 247); to his remark that Nature betrays what in mechanical invention would be "the *blunders* of numerous workmen" (p. 426), &c. (pp. 154, 104), as well as to chapters xii., xiii., and xiv. *passim*. Also to a series of quotations of another category touching a blacker matter than want of wisdom: to his

recognition of the fact that nature evolves contrivances "*abhorrent to our ideas of fitness*" (p. 415); to his remark that "to my imagination it is *far more satisfactory* to look at such instincts as the young cuckoo ejecting its foster brothers—ants making slaves—the larvæ of ichneumonidæ feeding within the live bodies of caterpillars—['cats playing with mice, otters and cormorants with living fish'] not as specially endowed or created instincts" (p. 234 and "Posthumous Essay on Instinct"; to his implication that the existence of "many instincts causing other animals *to suffer*" is incompatible with special creation (p. 417); to his letter to N. D. Doedes; to his affirmation that he was with Dr. Ludwig Büchner and Dr. Edward Aveling—avowed Atheists—in thought, though preferring the term "Agnostic," the date of his deprivation of God, or the knowledge of God, or whatever else a conversion to Agnosticism denotes, being, however, not specified. Perhaps, indeed, Darwinism is theistic after all. The remark that certain characteristics of the *Equidæ* make "the works of God a mere mockery and deception" if "each equine species were independently created" (p. 130), and that if natural selection operate widely and long "may we not believe that a living optical instrument might be formed as superior to one of glass as the works of the Creator are to those of man?" (p. 146); and sundry passages in the "Descent of Man" are indications of theistic belief. And, indeed, "we may console ourselves" for "the happy survive" (p. 61) "from the war of nature, from famine and death, the most exalted object we are capable of conceiving, namely, the production of the higher animals, directly follows" (p. 429). Mr. Darwin would have praised a man who picked pockets in order to pay his creditors ten shillings in the pound.

8. Mr. Darwin has "nothing to do with the origin of the mental powers," being "only concerned with the diversities of instinct and of the other mental faculties in animals of *the same class*" (p. 205). Seeing, however, that "what manner the mental powers were first developed *in the lowest organisms* is as hopeless an enquiry as how itself first originated, although the first dawnings of intelligence, according to Mr. Herbert Spencer, have been developed through the multiplication and co-ordination of reflex actions" ("Descent of Man," pp. 66, 67); and seeing that "*all* the most complex and wonderful instincts have originated" by natural selection (p. 206); we may infer accordingly the calibre of the prototypal instincts.

9. Mr. Darwin "placed in a most conspicuous position—

namely, at the close of the introduction—the following words: "I am convinced that natural selection has 'been the main, but not the exclusive, means of modification.'" (p. 421). Of course, by totally ignoring the ancillary factors an amusing picture of Darwinism can most readily be drawn.

10. Of the principle of selection by man the "importance consists in the great effect produced by the accumulation in one direction, during successive generations, of differences *absolutely inappreciable* to an uneducated eye—differences which I, for one, have vainly attempted to appreciate" (p. 23; to same effect, ii., 177). Let "no man say that any character is too trifling to be methodically attended to" (ii., 195). The production of new breeds "through the action of unconscious and methodical selection, has been *almost insensibly slow*" (ii., 231). Natural selection assimilates unconscious rather than methodical selection; it is more important for our purpose (p. 25). Methodical selection acts comparatively quickly, unconscious selection slowly (p. 32), natural selection presumably still slower (p. 270). "The slow and *insensible* changes [of unconscious selection] can never be recognised unless actual measurements or careful drawings of the breed have been made long ago, which may serve for comparison" (p. 25). Variation under nature has been slow in comparison with variation under domestication; "slight variations" occur "in a lesser degree" under nature (p. 62), and man is not so rigid in his selection as nature (p. 65); "under domestication, it may be truly said that the whole organisation *becomes*, in some degree plastic (p. 62) to same effect, pp. 9, 201, ii., 414; domesticated organisms are *much more liable to slight deviations of structures*, and to monstrosities, than species living under their natural conditions" (ii., 413, also p. 106); "no one doubts" this (ii., 241). "Some animals and plants withstand domestication or cultivation, and vary very slightly, *perhaps hardly more than in a state of nature*" (pp. 7-8). Thus the goose has a "small capacity of variability" (ii., 401; i., 303); yet the goose varies more than almost any wild bird (ii., 241). Birds, in fact, "vary extremely little" (p. 120).

We shall presently see that the ultimate cause of variation is the action of changing conditions. Now, domesticated animals can hardly have been exposed to greater changes of life than have many natural species during the incessant geological, geographical, and climatal change to which the world has been subject; but domesticated pro-

ductions will have often have been exposed to more sudden and to less continuously uniform conditions" (ii., 401). We shall also presently see that Mr. Darwin considers lasting rather than sudden influence to be the inducement of the tendency to vary.

For further evidence that "natural selection acts *only* by the preservation and accumulation of small" (p. 75)—["*infinitesimally* small" ed. i. p. 95] variations, consult the works cited *passim*.

The case is similar as regards instincts; that is to say, "no complex instinct can possibly be produced through natural selection, except by the slow and gradual accumulation of numerous slight, yet profitable variations" (p. 207).

"With respect to the lapse of time not having been sufficient for the assumed amount of organic change . . . this objection, as urged by Sir William Thomson, is probably one of the gravest as yet advanced" (p. 409).

11. In proof of this I may refer to works cited, *passim*; to the ever-recurring phrase "*accumulation* of slight *successive* variations"; to the constant insisting that "there is no necessity for supposing" simultaneity; to his distinction between initial variation and final co-adaptation (ii., 312), and to section 7 *ante*; for the question of co-ordination may be "put out of court" if there be no co-ordinator. Maybe co-ordination does occur now and then—that is, a variation would fail to benefit were it to occur alone, or a prejudicial variation be neutralised by a concomitant one—but it would be accidental—a word often used and well defended by Mr. Darwin; a word like "slight" and "accumulation," which is one of the vertebræ of Darwinism.

12. This is a very important section, and is really one with Section 14.

"The many slight differences which appear in the offspring *of the same parents*, or which it may be presumed have thus arisen, . . . may be called individual differences" (p. 34), and Mr. Darwin "looks at individual differences, though of small interest to the systematist, as *of the highest importance* for us, as being the first steps towards such slight varieties" as ultimately become species (p. 41). "Under the term of 'variations' it must never be forgotten that *mere individual differences* are included" (p. 64, also p. 80); and all "the individuals of the same species differ in some slight degree from each other" (p. 84). We read of variations "useful to *each being's own* welfare" (p. 102); "if any *one being* varies ever so little" it may supplant "some other inhabitant" (p. 143). *Confer* ii., 276. This individual variability

is "shown by every wild animal knowing its mate" (ii., 240), by a chill affecting some men with coughs or colds, others with rheumatism, &c. (pp. 6, 7). *Confer* ii., 279, and for further evidence, as heretofore, to works cited, *passim*.

Mr. Darwin perceived the liability of the obliteration of these variations. In former editions he "*spoke*" as if the preservation of "any single strongly-marked variation" "had frequently occurred," though he "*saw*" that such preservation would be "*a rare event*" (p. 71). "Nevertheless until reading an able and valuable article in the 'North British Review' (1867) [he] did not appreciate *how rarely* single variations, *whether slight or* strongly marked, could be perpetuated" (*ibid.*). Another event, however, occurs "*often*" (p. 72, also ii., 345), namely, "all the individuals of the same species have been similarly modified"—of this there can "be little doubt." "Or only a third, fifth, or tenth part of the individuals may have been thus affected, of which fact several instances could be given. Thus Graba estimates that about one-fifth of the guillemots of the Faroe Islands consist of a variety so well marked that it was formerly ranked as a distinct species under the name of *Uria lacrymans*" (*ibid.*). Why *Uria lacrymans* was not evolved by the "accumulation," or other Darwinian processes, is not stated.

The successor to this characteristic paragraph commences by re-assuring us:—"To the effects of eliminating variations of all kinds I shall have to recur [the subject having already been once postponed (p. 34)]; but it may be here remembered that most animals and plants keep to their proper homes, and do not needlessly *wander about;* we see this even with migratory *birds*, which almost always return to the same spot." He subsequently recurs as promised—to remark that "intercrossing will chiefly affect those animals which unite for each birth and *wander much;* for instance, *birds* (p. 80). And there are "*classes* which unite for each birth and *wander much*" (p. 137).

Animals have not usually to wander any prodigious distance to secure a mate, it may be remarked.

Having admitted that free intercrossing will "check, by blending together, any slight deviation" (p. 11), in summing up he finds that "the result will often be greatly retarded by free intercrossing" (p. 84). In the "Variation under Domestication" he considers the effects "so obvious that they need not be discussed at much length (ii., 62), for "with respect to the very slight differences by which individuals of the same sub-variety, or even of allied varieties, are charac-

terised, it is obvious that free intercrossing would soon obliterate such small distinctions" (ii., 67). The obliteration is, however, "governed by natural selection,"—a proposition perhaps more convenient than intelligible. May it mean that natural selection would favour a varying individual which happened to be pre-potent in the transmission of character? But for such pre-potency there is no guarantee. Another antidote to obliteration is, however, forthcoming,—namely, the tendency of allies to vary in a like manner. Was *the bearing* of this ever recognised by Mr. Darwin?

It better ensures the propagation of a new character if both parents are similarly characterised (i., 460),—"in the great majority of cases a new character . . . is not strongly inherited;" judicious matching is necessary (ii., 177). "In a *few rare* cases peculiarities fail to be inherited, apparently from the force of inheritance being too strong" (i. 465; ii., 58); but when any deviation in structure or constitution is common to both parents, this is often transmitted in an augmented degree to the offspring" (ii., 237).

Let it be "remembered that selection may be applied *to the family*" (p. 230). To obviate the dangers of the breeding of near kin, yet preserve the integrity of the new character, more than one family might advantageously be modified. Happily, then, "we may conclude from the analogy of *ordinary* variations" that "*some few*" are affected (p. 231), or "a third, fifth, or tenth part of the individuals may have been" similarly affected (p. 70). When all or nearly all are affected the result is referable to the direct and definite action of changing conditions (p. 106; ii., 260, &c.); but this is an ancillary factor, or rather (with apologies for sarcasm) a very ancillary factor.

13. The first question is not—Are variations constantly occurring? but, if so, are they being constantly selected? If the variations are of indifferent value, certainly not; but if they favour the possessor the circumstance will determine their selection. Now variation ordinarily being indefinite or promiscuous, the chances are against a succession of indifferent variations. Under domestication any desired character is almost sure to appear (pp. 22, 23, and compare Vilmorin's view, ii., 250). The only remaining factor, therefore, is the struggle; and as "each area is already fully stocked with inhabitants" (pp. 54, 85), while "we do not know that even the most prolific area is fully stocked with specific forms" (p. 101); and as natural selection is "the doctrine of Malthus applied with manifold force to the whole animal

and vegetable kingdoms" (p. 50); and as "it may metaphorically be said that natural selection is daily and hourly scrutinising, throughout the world, the slightest variations" (p. 65); and as organisms "have to struggle for existence from the hour of their birth to that of their death" (ii., 219), one would, variation granted, anticipate incessant selection.

It appears, however, that "the war of Nature is not incessant" (p. 61), and also (from the text of the diagram) that variations occur "often after long intervals of time" (p. 90), notwithstanding the fact that there is much superficial probability in the theory that variation is necessarily contingent on reproduction (ii., 239, 240). Moreover, although "mere lapse of time by itself does nothing" (p. 82, &c.), yet every form will, during each successive age, have to be slightly modified" (p. 308).

An Exegesis of the diagram may illuminate matters. Its construction betrays care. The relative length of B, C, D with G, H, and B, C, D and G, H *inter se;* the extension of F to F^{14} rather than of E to E^{14}, correspond with the text. If of a dozen species, a couple alone were ever beset with favourable variability never witnessed by their fellow-species during an aggregate existence of 36,000 generations; and if by an acceleration of variability the fourteen new forms were ever evolved from five in less than half the number of generations taken by the five to diverge from two, then the diagram corresponds with Nature.

14. "We clearly see that the nature of the conditions is of subordinate importance, in comparison with the nature of the organism, in determining each particular form of variation; perhaps of not more importance than the nature of the spark, by which a mass of combustible matter is ignited, has in determining the nature of the flames" (p. 8, ii., 281, &c.). As already stated, this section is really one with 12.

15. "Every variation is either directly or indirectly caused by some change in the surrounding conditions" (ii., 415). "These several considerations alone render it probable that variability of every kind is directly or indirectly caused by changed conditions of life. Or, to put the case under another point of view, if it were possible to expose all the individuals of a species during many generations to absolutely uniform conditions of life, there would be no variability" (ii., 242). Such changes, however, operate as a rule not upon the varying individual itself, but upon the parents or their reproductive organs (ii., 35, p. 260; *cf.* ii., 257),

while probably the commonest mode of action is upon successive generations ("two, three, or more," ii., 258); "changed conditions requiring a long time for their cumulative action" (ii., 39, &c.), it is "more especially its ancestors" who are influenced (ii., 241).

There is "some probability in the view propounded by Andrew Knight, that this variability may be partly connected with excess of food (p. 5). "Of all the causes which induce variability, excess of food, whether or not changed in nature, is probably the most important" (ii., 244).

I apprehend that—whatever may have determined the site of Mr. Darwin's TOMB—a recognition of the service rendered to Science by the promulgation of this theory, that from one to at most ten pre-Cambrian, presumably unicellular, organisms, created and vivified by the Creator's breath, have arisen without any subsequent interference, and we can hardly believe otherwise than without beneficent guidance, the structures and the bodily and mental activities of all organisms, by the accumulation, mainly by natural selection, of variations so minute as to be appreciable only to well-trained eyes, occurring in correlation but not in coordination with related variations, affecting single individuals, inducing intermittent modification, and being determined in their nature by an innate idiosyncratic plasticity, the chief ultimate cause of which is probably the accumulating action of changing conditions upon the parents, but more especially remoter ancestors, of the varying individual,—contributes to place his STATUE in the British Museum (Natural History); and that should any characterisation of this theory and its advocacy by its author and his adherents with justice assimilate, or even approximate, in severity such remarks as may be fitly made concerning the Bacon and Leibnitz quotations, the promoters of the memorial may some day be ashamed.

V. OUR MEASURES AND OUR STANDARDS.

By Louis D'A. Jackson, C.E.

THESE two subjects, though certainly inter-dependent to a very great degree, may be considered distinct and separate; or, if the whole be treated as one subject, the distinctions in its two branches should be perpetually borne in mind, more especially in reference to practical effect.

Our Measures are, for all purposes of measuring and weighing (that is, measuring by weight), the best existing national set of measures in the whole world, for the reason that a suitable and convenient Unit is provided for every ordinary want and purpose—a grand advantage that preponderates over all defects. Our Standards, or, to speak more precisely, the arrangements of standards, are worse than barbarous; for they constitute a spoilt and degraded barbarism, in which the simplicity, the forethought, and the scientific beauty of the original system have been vitiated or obscured.

The question then arises, "Does progress then consist in revivalism?" and it is difficult to answer it in the affirmative without much reserve and many limitations.

But before proceeding to treat of the need of these limitations, and their inducing causes, let us also notice the grand difficulty in dealing with the one subject singly or as a whole. It is a dilemma of the following form:—

With the masses our Measures constitute an inheritance of the same sort as our language, our customs, manners, habits, and institutions; and this reason is alone sufficient to deter them from parting with any of them, while a sweeping change would be strongly resisted. Hence if scientific men, understanding the want of system in our Standards, in their mode of determination and construction, and in their inter-dependence, should propose any interference with them in any way, the undiscriminating public immediately jumps at the conclusion that the existing measures will be altered. It certainly might be so, though we shall later show that it is not inherently necessary that it should be, and, besides, explain that it can be avoided through the exercise of some additional care and management. But the *prima facie* argument against all such improvement generally resolves itself into "Hands off from

the Measures, as you would create more work for the Standards Office"; or, "Do not interfere with the Standards, we cannot spoil the Measures of the nation."

The dilemma is apparently triumphant for the anti-progressists; actually it is not. Beyond it comes the insufficient argument "Let well alone," to which we reply we are willing to do so, but do not agree to "let bad alone." Also there is Mr. Gladstone's argument: "We have at present more important matters requiring attention," &c.—a plea for perpetual inaction with regard to any subject at all periods, that fails to justify dawdling and neglect. This, too, forces us to the conclusion that a permanent Board of Examiners should be specially constituted to deal with the large number of measures affecting the public welfare, convenience, subsistence, and progressive development, that are not sufficiently interesting to parliamentarian and party politicians.

But to return to the main subject,—the amount of necessary improvement in our Standards and their systematisation.

A mere collection of Standards, as simple representative units of measure for purposes of reference in comparison, does not satisfy the just demands of the nation; it is necessary that their inter-dependence throughout should be formed on some clear simple principles, in order to frame a system; otherwise we cannot compute or pursue calculations involving units of different sorts without needless labour and risk of error. Now calculating is as much a necessity of existence as measuring, and a system of Standards should be rendered fully suited to both purposes, for twenty-six millions of human beings should not be condemned to perpetual needless calculation, whether it be on the binary or on the decimal system.

The principal hindrance to easy calculation consists in the want of simple inter-dependence between the various classes of measures—the classes of length, surface (cubic or solid), weight, and capacity; hence when we proceed to compound classes—as power, pressure, irrigation, and many other compound terms of importance in Statistics, Returns, &c.—the difficulties cease to be inconveniences; they become serious stumbling-blocks, which cannot be removed until the first five classes are properly systematised.

There is only one general method under which this can be done—the geometrical method.

Some simple standard unit of length, when squared, should form a standard unit of surface; when cubed, should form a standard unit of cubic measure when solid, and a

standard unit of capacity when hollow; fifthly, the corresponding basic unit of weight should be the contents of the unit of solid or hollow measure, in some simple substance suited to the purpose, either solid or liquid.

The first point is, "Have we already any unit geometrically dealt with in the mode above described?" The categorical answer is, "No." The more correct answer is, "Nearly so, but spoilt by the mode of determining the Standards; and it exists in only one instance."

We have a foot, a square foot (perhaps, though there is some doubt as to its visibility); the cubic foot in a solid form may also exist; there is a capacity foot, in the form of a cylindrical metal bottle, termed a cubic foot measure; fifthly, there is Miller's foot-weight, or a bronze weight that is supposed to represent the weight of a cubic foot of distilled water at its utmost density, though it actually does not.

Our five Standard Units are a miserably imperfect and incorrect set: if the object had been to scamp the work of construction and comparison as much as possible, that object is realised. We do not even know with any certainty whether the simple foot in length is correct to old patterns, or, if so, to how many and to which? Probably it is merely the third of a yard shown on an old parchment record in which the dividing lines are tolerably large holes. The square foot might perhaps not be very useful if made to a correct standard, but it or some multiple of it might sometimes be of use, and should certainly be at least constructed as a Standard for reference, as well as for affording facilities in constructing a true standard cubic foot. The solid cubic foot may exist as a Standard in metal, but it is very doubtful whether it exists in glass or in quartz, or any non-corrosible material, so as to be fit for the ulterior object—that of aiding in the direct construction of the capacity foot or hollow cubic foot, and in that of the foot-weight. The hollow cubic foot, which should form the basic standard of capacity measure, certainly does not exist in the form of a metallic cylinder, resembling a tea-urn, but apparently not at all in its proper cubic form. It also appears that it was never constructed direct from the linear foot—that was perhaps too much trouble: the construction was based on a computation of what it *should be*, and perhaps also on an erroneous foot-weight; in other words, the direct construction was entirely avoided. Miller's foot-weight, as everyone knows, was not made from an actual cubic foot of water; he simply made a bronze weight of 62·32106 pounds avoirdupois, a quantity he had calculated to represent it.

We notice that direct construction has been avoided as much as possible in these Five Basic Standards; the only remedy is to have them made again, without this painful scamping of work, and the sooner it is commmenced the sooner we shall have a calculable system to deal with.

There are two points that require further comment. First, that of the comparison of weight and capacity and cubic measure by distilled water at its maximum density. In the abstract it is perhaps not the best means; it is however an admitted mode with which the public are perfectly familiar; it is also supported by the prescribed custom of denoting specific gravities of solids and liquids by comparison with that of distilled water at its utmost density. These two advantages tell heavily in favour of its retention for the present. Secondly, as to standard temperature; there's no doubt that two temperatures, one for the vessel or units of material, and another for the water used, constitute a complication. One single temperature should be used throughout for everything. Every possible source of derangement in construction should be avoided, as well as that of falling back on computation; and as air-displacement is to be avoided, a vacuum becomes necessary during some of the work.

Imagining now that our Five Basic Standards shall be correctly reconstructed, we shall then not have altered any of our measures. This is generally admitted for the reason that the changes effected will be small, perhaps $\frac{1}{2}$ per cent, and the trading communities and the masses do not object to this—they do not call it an alteration but a re-adjustment which they would quite ignore, although it would be a boon to scientific men.

The ratios of all our commercial units with regard to these Five Basic Standards can then be accurately determined in their five several classes. However incongruous they may be, we shall no longer be kept in the dark about their true relations to each other throughout; the result will be a system instead of a collection.

Are we to end there? Not quite. First, we now have, it is true, an Act permitting decimalisation on any unit of English measures, but this Act is rendered nugatory by other Acts that make non-decimalised units compulsory, as, for instance, compelling distances to be mentioned in miles, furlongs, and yards; three units in each distance; similarly with other things. We require full effect to be given to the Permissive Act; our calculations can then be simple and comparatively easy, even when the compound units of modern invention are dealt with. While the Measures of Trade thus

remain practically unaltered, the scientific man and the calculator will be able to set them on one side in most of his work, without the degradation of adopting French fashions.

Next as to the multiples and submultiples of our commercial units. They will probably fall eventually into a mixed decimal and binary scale, formed on the *to be improved* five basic units, at some time when the masses and the tradesmen require this change; but it is not the duty of scientific men to press it on them against their will. Why should any interference with them be necessary? The object of this article has been to prove that it is not. The connection between the foot-weight and the ounce avoirdupois being millesimal, or very nearly so, this advantage will probably be utilised and enlarged upon in future development, not only by the scientific man, but also for commercial purposes.

Having thus run through a practical proposal, we can see the difficulties of anything like a complete revivalism of our very ancient measures at the present time. We are forced into legislating for ourselves and for our own time, whenever we can legislate at all.

ANALYSES OF BOOKS.

Hygiene; its Principles as applied to Public Health. By E. F. Willoughby, M.B. London and Glasgow: W. Collins, Sons, and Co., Lim.

This work contains so much matter of sterling excellence that we are tempted to overlook the beginning and end of the preface, in which the author bows down to the educational Moloch of the present day.

The first section of the book deals with dietetics. Here, in speaking of the carbohydrates, the author might usefully have referred to the doubts which have lately arisen whether beet-root sugar is really identical with the saccharose of the cane, or whether it is not a distinct product, betose, possessing the same percentage composition, but differing physically and physiologically.

The question on what the efficacy of anti-scorbutics depends is discussed at some length, and is left, from a theoretical point of view, undecided, though, as the author remarks, no difficulty is felt in practice. May not a part of the immunity which sea-faring men enjoy in comparison with their predecessors be due to the fact that sea-voyages are now very much shorter than was the case before the introduction of steam?

Dr. Willoughby asserts that potatoes contain no acid. But if he will cut a raw potato, and apply a piece of blue litmus-paper to the freshly-cut surface, he will be convinced of his mistake.

In considering the potential energy contained in food the author shows the erroneous nature of Frankland's estimates. He quotes this chemist's figures, however, drily remarking that "questions on them may be given in examination-papers." He adds that "since it has been proved that starch is more easily metabolised than fat, the fancied superiority of the latter as a source of heat must be rejected as erroneous."

Few among the general public will be prepared to hear that veal-broth contains more nutriment than mutton-broth or beef-tea. It appears that "at the Munich hospitals, where dietetics are studied more scientifically, perhaps, than anywhere else, veal minced and cooked with meal is much used for convalescents."

Concerning the cereals Dr. Willoughby states that a great part of rye-bread, when eaten, passes off undigested. Oatmeal was, he considers, an excellent food so long as milk could be had *ad libitum*, but without this accessory it is much less valuable. Maize he pronounces to be, of all the cereals, the nearest

approach to a perfect food. It is to be regretted that obstinate prejudice prevents its use for human food in Britain. The potato he places low in the dietary scale, and deplores the fact that English people look on all other vegetables as mere luxuries. The lentil he considers the most nutritious, as well as the most digestible, of all forms of vegetable food.

When speaking of alcoholic drinks, the author points out that no natural wine can contain more than 14 per cent of alcohol, since at this point fermentation is arrested. He denounces the increasing use of the rank, coarse potato-spirit of Prussia, loaded with "fusel" for the fortification of natural wines and the manufacture of totally spurious ones.

On the subject of alcoholic drinks Dr. Willoughby takes a judicious mean path. Whilst utterly condemning their common random use, especially on an empty stomach, he insists that they have their time and place. Alcohol "undoubtedly checks metabolism, and much nonsense has been written on this point, even by such men as Dr. A. Carpenter, as if this process could not be too active." The fallacious character of the conclusions of Lallemand, Perrin, and Duroy is shown. These experimentalists, "if they deserve the name, dosed men and other animals with enormous quantities of alcohol, and because they could detect a certain quantity in the urine, &c., inferred that none of it was consumed."

Cocoa, when freed from a portion of its fat, the author considers almost a perfect food, but he deprecates the common practice of mixing it with starch.

He protests against the common and increasing use of ice and iced water, which interfere with digestion, and may even give a serious shock to the nerve centres. At the same time he exposes the fallacy that ice-water is always pure. It once fell to our lot to examine the feasibility of a scheme for purifying the sewage of large towns by freezing. We found that the ice retained a large share, not only of the suspended, but of the dissolved pollutions.

The nutritive value of Liebig's extract of meat is pronounced less than nothing, since its constituents actually accelerate metabolism.

On the use of milk the author gives the caution that foot and mouth disease is directly communicable by this secretion. As regards tubercular disease the evidence is less decisive, though there is strong reason to believe that danger really exists if the milk is used regularly. The flesh of animals "should be condemned unconditionally when evidence is had of cattle plague, epizootic pneumonia, sheep-pox, acute rheumatism, pig-typhoid and scarlatina, erysipelas, anthrax, and trichinæ."

Under the head of adulteration our author shows the shortcoming of the existing laws. Thus no action can be taken against the vendors of the so-called "French Coffee," or "Coffee as in France,"—the most disgraceful falsehood uttered since Chaucer

first denounced a shopkeeper as "much a liar"—though 75 per cent of the article is chicory, and not coffee at all."

In speaking of "butterine" Dr. Willoughby makes no mention of the circumstance that the fat of diseased animals is often used in its manufacture without having been subjected to a temperature sufficiently high to ensure the destruction of microbia.

On the subject of water we meet with the very questionable statement that the water supply of Paris far surpasses that of London in purity. At the present time there are complaints of fæcal matters present in the Paris water, and the question is raised of a fresh supply from some distant springs.

The author does not accept the statement that a river, once polluted with sewage, is incapable of self-purificaion, and gives an official account of the state of the Seine, above and below Paris, to prove his case. He holds—and justly—that "a mere trace of albumenoid ammonia derived from enteric and choleraic stools teeming with bacteria may be more dangerous than a hundred times the weight of healthy fæces, or other animal or vegetable matter."

We regret that space does not allow us to carry any further our examination of this most valuable work, which is in small compass an encyclopædia of public health.

Chemical and Physical Analysis of Milk, Condensed Milk, and Infants' Milk-Foods, with special regard to Hygiene and Sanitary Milk Inspection. A Laboratory Guide, developed from Practical Experience, intended for Chemists, Physicians, Sanitarians, Students, &c. By Dr. NICHOLAS GERBER. Translated from the Revised German Edition and Edited by Dr. HERMANN ENDEMANN. New York: the Author. London: Trübner and Co.

THE author of this valuable manual, judging from his preface, makes a point of rejecting the methods for milk analysis known and used prior to 1877, and proposes others as superior in accuracy, simplicity, and economy of time.

Dr. Gerber enters upon his task by giving a description of normal cow's milk, and noticing the physiological and other causes influencing the character of the secretion. Here it may be remarked that in spite of the great attention which has been drawn to a subject of so great agricultural and hygienic importance, all the circumstances of the case have not been fully studied. Thus, concerning the changes which occur in milk when cows are in heat, the author admits that but little is known. He informs us that after a certain age the yield of milk diminishes,

but he does not—as far as we see—throw any light upon the possible changes in its quality. This is the more to be regretted, since the opinion prevails that the milk of aged cows has a tendency to occasion phthisis. Decisive information on this point would have been very valuable.

The influence of meteorological phenomena on the yield and quality of milk, we are told, has yet to be examined. "Hard work," it is stated, impairs the quality and reduces the quantity of milk. This is a consideration which has no practical interest for the English reader.

In a section on the colour of milk it is remarked that deep blue spots are sometimes produced in milk by a certain fungus. The pigment generated appears to border closely upon triphenylrosaniline.

As to the microscopical examination of milk, the author considers that it can merely furnish useful indications for the detection of morbid changes or for the determination of adulterants. The lower organisms present in milk are *Mycoderma lactis*, *Mucor racemosus*, *Penicillium glaucum*, *Dictyostelium mucoroides*, and various species of bacteria, vibrio, zoogloea, and monads. Hence no one need wonder that milk may be the medium of transmitting infectious diseases. It is thoroughly established that microbia present in the water drunk by cows may reappear in their milk. Other channels of infection are impure water used to adulterate milk, or even to wash out the cans. Milk whilst stored in dairies has been found to absorb and communicate the virus of typhus and of scarlatina. Dr. Gerber is of opinion that the milk of cows suffering from foot and mouth disease and from cattle plague is not rendered safe by boiling. He protests against the use, in the diet of milch cows, of distillery swill, brewers' grains, and other fermenting matter. He rightly suggests that the supervision of milk should extend to the animals themselves, their food, water, and housing.

As regards adulteration, the author is of opinion that dealers mostly confine their operations to watering and to the addition of skim-milk to new milk.

Concerning sheeps' milk we find the following curious remark:—"The many points which mountain dairies and the dairies of the marshy, flat lands have in common is also illustrated by the fact that sheep are kept for their milk in Holland."

In speaking of condensed milk the author exposes a vulgar error. He writes—"It is simply a prejudice that Swiss condensed milk should, as regards richness, possess superior qualities. Considerable stress is laid on the superior quality, especially as regards flavour, of the milk of cattle fed on Alp mountain pasture. If we consider, however, that the establishments which manufacture condensed milk are not only situate in the valleys, but use exclusively, as they do everywhere else, milk produced in the valleys, the ridiculousness of the claims made are at once apparent."

It is stated that among the varieties of condensed milk is one in which salt is partially substituted for sugar. In France there is even, *horribile dictu*, milk condensed with the addition of tar, coal or wood!

In the instructions for examining human milk we find it stated that "Woman's milk differs from the milk of animals, especially cows." We must here ask whether the milk of animals belonging to the order of Primates, and especially of the anthropoid apes, has ever been fully examined?

The author's suggested rules for a rational milk inspection are admirable, and we should greatly like to see them officially adopted in England.

The only defect of this book is that it is written in very imperfect English.

Expository Thoughts on the Creation. By JAMES ROBERT SMITH. London: Elliot Stock.

WE have here one of the many attempts at reconciling the teachings of modern Science with the Hebrew cosmogony. The author confesses to a lack of sufficient knowledge of Natural History, and pleads that neither leisure nor learning (properly so called) has been brought to bear upon his undertaking. He admits, further, being "no Hebrew scholar."

In an early part of Mr. Smith's treatise we further find him recording his opinion that "the Bible was not written to teach Nature's facts and laws and workings." This is the very view which, since the days of Giordano Bruno, and of Galilei, has been held by men of Science in general. Rightly understood, this view renders all inquiries as to the harmony and disharmony between Science and Revelation a mere waste of time. Mr. Smith, however, does not feel the the full force of the admission, or he would—rather he *could*—have proceeded no further.

The first chapter, entitled "Observations and Conclusions respecting the Authors of Creation," lies entirely outside our competence. We can merely intimate that the author takes up a position which a few centuries ago would have been perilous in the extreme. Even in our days it may give occasion to much inkshed should it attract the attention of professional theologians.

In the second chapter Mr. Smith takes up what he calls "the much-vexed question respecting the length of each period translated in our English Bible 'day.'" To the man of Science who takes his stand on the simple principle above laid down, this is no question at all. The author conceives, as have done others

before him, that the days of creation were periods of considerable length, to each of which he assigns the length of 100,000 years. The reasons assigned for selecting this precise length of time seem to us highly fanciful. Thus we are told that the five ciphers placed at the right hand of "the emblematical unit 7," in order to give the earth's total existence of 700,000, "may symbolise the five senses of all the higher genera of animals, as well as the elements of general nature, inanimate and animate and organic." The "compound elements" of Nature were aëriform vesicular matter, wind, watery vesicular matter, oleaginous vesicular matter, and salt."

To find wind classed as an element is indeed bewildering, and we ask, though in vain, for any evidence upon which such a classification can be built.

In succeeding chapters we find, however, much more that is perplexing. Thus we read of "the primordial pair of the second genus of metals, namely, copper and brass." "Taking lead and tin, copper and brass, pig-iron and dense iron as being respectively natural pairs, as I contend that they are, lead, copper, and pig-iron were accordingly formulated in the northern hemisphere, and tin, brass, and dense iron in the southern." Leaving out any remarks on finding the well-known alloy brass classed with simple metals, we may well ask in what part of the southern hemisphere it occurs in Nature? We may likewise ask whether, surface for surface, copper is not equally plentiful in the south as in the north?

The mystery is how any man of evident culture, and of thoughtful, intellectual habits, can entertain such notions as we find scattered on almost every page of the work before us? This difficulty hinges, doubtless, on the peculiarity of English education, which, until lately at least, allowed the middle and upper classes to grow up in the grossest ignorance of *things* in contradistinction to phrases.

Journal and Proceedings of the Royal Society of New South Wales for 1883. Sydney: Richards. London: Trübner and Co.

THE Anniversary Address, by the President, Christopher Rolleston, contains a feature which will now appear strange to many persons in the Home Kingdom,—we mean the most eloquent tribute to the memory of Charles Darwin. So much rubbish—we use the word advisedly—is now obtruded upon the world that even the death of our foremost *savant* and the mighty lessons of his life are fading into obscurity. We have heard it

hinted that had the "Origin of Species" been first published at any time during the last eight or nine years it would have met with little attention, save among professed naturalists.

We must call particular attention to a paper by Mr. Peter Beveridge, on the "Aborigines inhabiting the Great Lacustrine and Riverine Depressions of the Lower Murray, Lower Murrumbidgee, Lower Lachlar, and Lower Darling." It is here remarked that "a sting from a deaf adder is considered by the natives hopelessly fatal; therefore they rarely attempt the extraction of the deadly virus injected by that reptile's horny tail spur: in fact they have not any time to try a cure, for the victim seldom lives twenty minutes after being wounded. These reptiles are the most dreaded of all the snake kind by the aborigines, on account of their superior virulency. To add to the danger arising from these reptiles, nothing will move them from the position in which they are met. If one is touched by a careless foot, or even by a piece of stick, as he lies in the path, he does not crawl away from the interrupting object as quickly as possible, as is the manner of most reptiles; he merely raises his head and tail simultaneously, and, with the rapidity of thought, seizes the disturbing object with his mouth, holding firmly thereby, whilst he drives his tail spur into it repeatedly." Here, then, we have a nameless snake, fully as deadly as the cobra, but anomalous in as far as its venom is secreted and emitted not by glands in the head and hollow teeth, but by a special organ at the tail. Further investigation is here surely needed.

According to Prof. Liversidge, Mr. Brown, a Wesleyan missionary, brought from New Britain a soft white limestone, physically and chemically undistinguishable from chalk. He now exhibited specimens of flints from the islands of Ulana and Ugi;—an additional proof of the probable presence of true chalk of the Cretaceous age in the South Sea Islands.

British Fresh Water Algæ, exclusive of the Desmidieæ and Diatomaceæ. By M. C. Cooke, M.A., L.L.D., A.L.S. 2 vols., 8vo., pp. 330. Plates 130. London: Williams and Norgate, 1882 to 1884.

This magnificent work, of which the first two parts have already been noticed, has at length been completed, and there is no reason to alter the favourable opinion then expressed. ("Journal of Science," vol. 4, p. 263).

The separation of the "Fresh Water Algæ" from the numerous Marine species is purely a matter of convenience, and as the

author states, "is confessedly an artificial arrangement which demands apology, but can scarcely receive justification. The only excuse which can be offered is that it serves the purpose of t[illegible], for whom the present work was written, namely, the micro-[illegible]sts who desire some acquaintance with the organisms met [illegible] in their excursions to ponds and ditches. This class of [illegible]nts certainly deserve such assistance: their limited time for [illegible] is mostly spent at the microscope, or out of doors collecting [illegible]mens, rather than in searching through libraries and over-[illegible]ming masses of periodical literature. They work earnestly and yet leisurely, and not having to cram for passing examinations, dispense with the services of the professional coach; if the aid of someone knowing more than themselves is needed, it is found at one of the natural history societies which now abound all over the kingdom, and the strange specimen sooner or later reaches the hands of an expert, who is able to tell something about it, or still more humbly to say he does not know, and advise how further research is to be conducted. Those who have had the good fortune to meet the author of the present work either at the Quekett Club or at one of his excursions, will know how to value the instructions and hints of so genial a teacher—one who loves science for its own sake.

The introduction contains a concise history of the subject, from which it appears that in Ray's time about twenty species of Fresh Water Algæ were known; these had increased to eighty-eight at the publication of Dillwyn's "British Confervæ" in 1809.

The next important work was Hassall's "Fresh Water Algæ," published in 1845, containing descriptions and figures of two hundred and ninety-seven species. Since that time the student in search of information respecting these organisms must have sought for it through the over-increasing mass of scientific periodical literature. The present work contains descriptions of four hundred and thirty-eight species. Respecting the omission of the Desmidieæ, the author must answer for himself. "This large and interesting family is designedly excluded, as it is proposed to treat them separately. As so many students confine themselves exclusively to this family, this proposal will doubtless commend itself. The excellent text-book by J. Ralfs has long been the standard for English students, and would be so still but for its scarcity and the large number of additions in the interval since its publication." Similar remarks will apply to the more numerous family of Diatomaceæ. The coloured plates are beautifully executed, and in many instances are from the author's own drawings, in which case the magnifying power invariably accompanies the figure.

A list of the authorities quoted, and what is even more useful to the readers for whom the book is intended, a well-compiled glossary, form part of the work.

While congratulating the author on the completion of his book,

it must be acknowledged that, although it has come to an end, it is evidently not finished. No one can read it carefully without thinking that, although much has been done during the thirty-nine years which have passed since the publication of Hassall's book, as much, or more, remains to be accomplished. The Desmellaceæ, Protococcaceæ, and Chroococcaceæ will furnish plenty of work for years to come to any who will devote time and leisure to cultivating, watching, and working out their life-history. Such patient and exhaustive research as that of Messrs. Dallinger and Drysdale on the Monads, applied to the investigation of fungi of the lower and little known organisms, must, in time, yield equally valuable results. Many of the forms included in the above-named families will probably be found to be only immature forms of other algæ or lowly organised plants.

The Agnostic Annual, 1885: London: H. CATTELL AND CO.

AMONG the essays in this annual of intensely heretical repute four at least especially claim our notice. The first, and perhaps the foremost of these is "Pessimism and Physiology," by C. N.—a refutation of the philosophy of Schopenhauer and his imitators. The author does not accept the view that this is the worst of all possible worlds; but neither, as far as we can see, does he advance the inverse proposition. Either of these extreme views can be held only by non-observers who have never risen to the wisdom conveyed in the homely old proverb, "it's an ill wind which blows nobody good." We might ask both optimist and pessimist what world do they mean? The world as it has been, as it is, as it probably will be, or as it might be if man would put even his scanty present knowledge into practice? We see changes for the better, due to man's awakening intelligence and moral sense, and proving that the world cannot previously have been the best possible, since it has proved capable of emendation. We see, on the other hand, changes for the worse, due to man's greed and ignorance. We see forests destroyed and the climate of extensive regions altered for the worse by the simple introduction of goats—animals which, as Sir Joseph Hooker informs us, have occasioned far greater desolation than has been due to war. We see other countries made fit only for the dwelling of demons by the coal-smoke and other products of modern industrialism—proof sufficient that our planet previously cannot have been the worst of all possible worlds, since it has proved capable of so much and far more deterioration.

Or, if we may take the common pessimist formula that "life

is not worth living," there is room to ask "to whom?" Certainly not to all the many who are in harmony with the spirit of the present age, whose aim is high living, and low—very low—thinking, very well content with "business" during the day, and Little Bethel, or the tavern parlour, or the parliamentary debating society at night.

C. N. points out that "very little pleasure is felt in connection with continuous organic functions unless they have been interrupted by ill-health or abstinence. There is little delight in normal muscular activity, or in deep drawn breaths of fresh air, except to the invalid who has long been pent in a sick room."

Most true; indeed the portions of our bodies susceptible of pleasure are few and small, whilst the great rest exerts its functions when in health without our consciousness.

He continues: "The darkest, the most logical, the most absolutely immoral form of pessimism is sponsored by Calvin. But the exile of the middle ages is over long ago; the husks which the swine did eat are forgotten; even the good father is superannuated, and we are striving chiefly to retain faith in the fatted calf."

What a truthful picture!

He continues: "World-weariness sets in, and with it a new form of pessimism, more subtle, and perhaps more paralysing. Yet the doctrine of Schopenhauer can scarcely be called *new*, seeing that it is in reality a one-sided presentation of a religion more ancient than Christianity. Having worn out the first suit of mourning which we borrowed from the East, forthwith we proceed to borrow another, different in pattern, but equally dismal. St. Paul gives us over to Buddha." From the East!

> "Ach, des Lebens Blüthen sind verfallen
> Vor des Ostens melancholisch Wehen."

C. N. ably maintains that the existence and development of conscious life on the earth cannot be comprehended save on a theory the very opposite to that of Schopenhauer. "Evolution has been possible because sentient beings have, on the whole, liked life, and taken delight in the exercise of their faculties."

Under the strange title, "Mars' Hill in London," Mr. Moncure Conway notices the recent encounter between Mr. Herbert Spencer and Mr. F. Harrison, and the summing up of Mr. Justice Stephen. Mr. Spencer admits that the "Unknowable" has nothing necessarily moral about it. Mr. Harrison might make a still wider confession concerning his Positivism, with its new God "Humanity"—almost too absurd to be capable of serious discussion. For what shall we say of a creed whose apostle Comte abandoned his devoted wife, who had nursed him through an attack of insanity, and formed a *liaison* with the wife of a convict?

"Agnosticism and Theism" is a kind of manifesto by Dr. W.

B. Carpenter, who objects to the standpoint of Agnosticism as "defective because it takes account of the intellect only," yet if obliged to choose between Agnosticism and Orthodox Theology would decidedly prefer the former."

But this short essay is noteworthy chiefly for its unconscious, and therefore inexpugnable, egotism. Dr. Carpenter writes: "When Agnosticism shall have produced any such philanthropic worker as Mary Carpenter, it may claim to be a motive power for good. As her brother, intimately conversant with her springs of action, I feel certain that nothing but her religious faith could have carried her through her life of struggle with the dead weight of ignorance and prejudice she had to encounter." It is not every writer who would have sought among "his sisters, his cousins, and his aunts" for a telling example.

"The Messrs. Facing-both-ways of Science" by Dr. Aveling is a severe, and as it seems to us an unjustifiable, censure of such scientific men as do not proclaim themselves atheists. All such are referred to the *genus* Humbug and the *species* Facing-both-ways. Within this species the author recognises the sub-species *Scientificus*, comprising again two varieties, *Indifferens* and *Religiosus*. To the former variety are referred "the great mass of our scientific men. Examples are the late Charles Darwin, and, among the living, Ray Lankester. At first I had written the names of Huxley and Tyndall; but, on reflection, these gentlemen were struck out, for they are such hybrids that it is impossible to classify them anywhere. Tyndall in one breath delivers a Belfast address and in the next informs an amused world, after a performance suspiciously like a prayer meeting, that he recants. Huxley writes boldly against Theism, and is then moved to an almost tearful anger when his views are made public." Dr. Aveling continues: "The characteristics of the sub-species Indifferens are as follows: entire unbelief in the supernatural at heart; respectability, fear of society; conformity to its usages; the idea that a belief in the supernatural is necessary for the lower classes; silence upon the fundamental questions at issue between the Theistic and Atheistic world. . . . We also know what the majority of our professors think of the Christian religion, and yet we cannot get them to speak out."

It may here be asked how the author knows so exactly what the majority of our professors believe? If, further, "we all know" their opinions, what more need for "speaking out" can remain?

If we, for argument's sake, suppose that the bulk of scientific men are, at heart, entire disbelievers in the supernatural, we can very well imagine them concluding that the progress of science is of vastly more moment than the success of an Atheistic propaganda, and keeping silence in the knowledge that any such "speaking out" would discredit science with the majority

of the nation. We endorse the plea which Dr. Aveling puts into their mouths only to over-rule, *i.e.*, "Their calling is that of investigating natural phenomena, and the conclusions to which they may have come *re* the spiritual are of no moment to any but themselves." It is easy to say, "Men are turning to the teachers of science to-day as they turned to the teachers of religion in the past."

Now the great error of these teachers of religion was that they assumed to lay down the law on all things. Men of science, more prudently, claim no authority beyond the limit of their own special studies. Sometimes, indeed, a mathematician or a physicist attempts to adjudicate on biological questions, with results which are anything but encouraging.

Turn we now to Dr. Aveling's second variety—"(2) *Religiosus.* Examples: Owen (palæontological), Mivart, and Beale. Its characteristics are as follows:—Belief in Christianity; dexterity in extorting harmonies between science and revelation; blindness to logical conclusions; a Gargantua's mouth for contradictions; membership of the Victoria Institute. This variety would not exist but for the fact that the variety *Indifferens* is in being. Its individuals are in the main Sunday-school teachers with a smattering of scientific knowledge." It seems to us that the three *savants* just mentioned, to whom may be added Wallace among the living, and Faraday and Clerk Maxwell among the recently dead, possess something vastly more than a smattering of scientific knowledge, and we fail to see their resemblance to Sunday-school teachers. Nor do we see in the least how they would be moved to renounce Theism if Ray Lankester, Huxley, and the majority of our professors were publicly to avow themselves Atheists.

The whole of this essay gives the most signal proof of the ability of the author, for whom our concluding wish is that he may do not less in scientific research than the men whom he pronounces "humbugs."

Our Corner. Vol. iv., No. 5, November 1st, 1884.

THIS issue is, to us, barren. Those able scientific articles contributed to former numbers by Dr. Aveling and others are here conspicuous by their absence. The present time of political excitement is no less unfavourable to science than it is to industry and commerce.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

ELECTRICITY AND THE ANIMAL.

I HAVE read with great interest your remarks on Dr. Stone's paper on "Electricity and Health," which was read before the Society of Telegraph Engineers at the Health Exhibition; and also those on the experiments stated to have been made with artificial incubation, in the presence and absence of magnets.

At the meeting at the Healtheries I pointed out that probably the discrepancy in the measurements of the electrical resistance of Dr. Stone's 7 feet of clinical clerk, when measured with high and low tension currents, and more particularly the great apparent fall in the resistance when measured by means of the alternating current, might very probably arise from an opposing electro-motive force being measured as resistance.

I pointed out that in every part of the animal body we have an artery and a vein running side by side. When we pass an electric current through a portion of the body—say through a limb—the current passes along the two blood-vessels in the same direction with reference to the limb itself; but it must be remembered that, looking upon these vessels, in comparison with their surroundings, as conductors, each would create its own magnetic field in concentric circles round it, so long as the current was passing through it.

In each, however, the fluid blood—which is here the conductor—is, we know, in rapid motion, so that we may expect to have developed in each, seeing that it is a conductor moving in a magnetic field, an opposing electro-motive force, which I suggest is measured as resistance.

This supposition is borne out by the fact that with the alternating current, which we know creates but a comparatively feeble magnetic field, the measured resistance is strikingly reduced.

I believe I am correct in stating that we have no other case of a body altering its electrical resistance, without—either first or at the same time—altering some other physical condition, and therefore, it appears to me, we are bound to look further for a solution, and not to accept at once mere experimental facts, which do not agree, as proof of physical conditions in the animal body which do not exist in other bodies.

It may, of course, be objected that the magnetic field so cre-

ated must be exceedingly small. So it must; but the whole thing is very minute. The currents with which surgeons and others deal who apply electricity to the animal body are, I believe, the feeblest that are ever used. It must be remembered, too, that the conductors—the blood in the vessels—are moving very rapidly indeed.

I regard the experiment with the magnets and the eggs as bearing very strongly on the views I have put forward. Evidently the presence of the magnets produced some change in the embryo of sufficient importance, in many cases, to destroy life, and in others to interfere with the performance of the proper functions of the bird when hatched.

I suggest that the presence of the magnets in the neighbourhood of the eggs generated minute currents of electricity in the blood and other vessels, which acted detrimentally on the formation of the chick.

I would suggest, as an extension of the experiment, that magnets of different power be placed in the neighbourhood of separate eggs, sufficiently removed from each other to be independent in their action; and that the poles of the magnets, made small or large, as convenient, be placed in different positions with reference to each other and to the egg.

It appears to me that if the presence of a magnet can be made to act prejudicially on the embryo, it should also, by suitable arrangement, be possible to make it act beneficially.

SYDNEY F. WALKER, M.S.T.E and E.M.I.M.E.

THE OPTICAL EFFECTS OF AN UNUSUAL POSITION OF THE HEAD.

CAN any of your readers solve for me the following optical (or physiological?) problem? In my school-days, when taking a stroll with any of my companions, it was a common practice among us, on reaching any point where there was a wide prospect, to look at it bending downwards, and looking either between our knees or alongside one of them, the crown of the head being turned downwards. In this ungraceful posture the landscape appeared much more beautiful than when viewed in the ordinary upright position, everything appearing to have a warmer and richer colouration.

R. D.

[We should think that the effect must be due to a determination of blood to the head, caused by the unusual attitude.—ED. J. S.]

Contrary to our usual custom we give replies to the queries of the following two correspondents :—

1. "VESLE."—Dr. R. Angus Smith's process for the detection of microbia in water is as follows :—In 100 grms. of distilled water, heated to +30° C., dissolve 2 to 5 grms. of very dry gelatin in thin leaves, and 2 centigrms. of sodium phosphate. Filter and precipitate with fresh albumen. Take in a test-tube 25 c.c. of this solution ; add to it an equal volume of the water to be examined, and keep the tube thus filled for some minutes in water at +35° C., to effect an intimate mixture. Stopper the tube with cotton-wool, and keep it for a few days in a warm room.

2. R. M. D.—We have it on the authority of eminent counsel that no learned society is warranted in giving the title of "Fellow" to its members unless it is incorporated either under a Royal Charter or under a special Act of Parliament. You have therefore no right to place the letters "F *x y*" after your name.

NOTES.

At a recent meeting of the Metropolitan Board Teachers' Association, which consists of 2078 head and assistant masters and mistresses employed in 320 London Board Schools, it was resolved unanimously—"That the Metropolitan Board Teachers' Association endorses generally the report of Dr. Crichton Browne on over-pressure and worry in schools, and further thanks him for his Report."

Says "Science":—"Now, Spiritualism is an evil in the world. In America it is a subtle and stupendous evil; a secret and unacknowledged poison in many minds, a confessed disease in others,—a disease which is sometimes more repulsive to the untainted than leprosy."

The American Ornithologists' Union has done two very sensible things. It has decided that the European house-sparrow is not an eligible bird in North America, and it has appointed a Committee for the protection of American birds and their eggs from wanton destruction.

The Rev. J. G. Wood comes to the conclusion that the musk beetle (*Aromia moschata*) has the power of emitting or suppressing its odour at pleasure, but that when dying the scent is continuous and very powerful. He gives a case in which a scent of roses is diffused from the human body in fatigue and weakness. "M.A. (Oxon)," commenting on this statement in "Light," gives some similar cases. (Considering that the mere displacement of a molecule may convert a stench into a perfume, and *vice versâ*, we need scarcely feel surprised if the emanations from the dying or the dead are sometimes pleasantly scented.)

"Cosmos les Mondes" gives, from an American source, a case of a tinned copper reservoir having been perforated by microbia.

The statement that ants formally bury their dead fellow-citizens has been confirmed by certain writers in the "Neue Züricher Zeitung." A Miss Hatton seems to have even witnessed the funeral ceremony.

M. Marey ("Comptes Rendus") finds a simple explanation for the more rapid spread of the cholera after storms and heavy rains. The excreta of the patients are often carelessly thrown into fields, gardens, yards, and even streets. Heavy rain may

easily wash the infectious matter into brooks, walls, &c., and thus spread the infection.

M. R. B. Roosevelt (American Assoc.) gave cases of hybridism between different species of Salmonidæ. He proved that a fish hybridism by no means necessarily implies sterility.

Rev. E. Hill, F.G.S. (British Assoc.) shows reasons for rejecting all the theories of the Ice-age, and considers that further investigations on the last series of changes in the outline of the continents of the globe should be undertaken.

R. von Lindenfeld concludes that the Glacial Period in New Zealand must have been very recent.

According to Prof. Storer carbonates of lead and baryta in a pure state are fatal to mice, but the addition of a small quantity of whiting rendered them both innocuous.

A French statesman, speaking on behalf of the proposed general Exhibition to be held at Paris in 1889, gravely asserted that France had, in 1855, given the first example of an international "Exposition." What of England and the World's Fair in 1851? Thus is history manufactured.

"Cosmos les Mondes" is filled with indignation at the proposed international adoption of the meridian of Greenwich, and suggests, instead, Rome or Bethlehem!

Prof. B. G. Wilder (American Assoc.) proved, by photographs and a preparation of the brain of a chimpanzee, that in the anthropoid apes the cerebrum extends over the cerebellum by at least a millimetre.

M. Boillot ("Comptes Rendus") calculates the specific gravity of liquefied oxygen as 0·888.

Prof. C. S. Minot (American Assoc.) considers the ovum to be homologous with the encysted protozoon, the zona radiata being equivalent to the capsule or cyst of the protozoon, and the contents being also homologous.

The "Medical Press and Circular" has an exceedingly able article on the present Examination-Craze.

"Cosmos les Mondes," in a memoir on the "inconveniences of the baccalaureat," shows how rarely men of original minds have earned scholastic distinctions.

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